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AMERICAN SOCIETY OF CIVIL ENGINEERS

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PAPERS

SEDIMENTATION IN RESERVOIRS

By Berard J. Witzig,[1] Jun. Am. Soc. C. E.

Synopsis

The principal phases of the problem of reservoir sedimentation are reviewed in this paper, including (*a*) the origin and nature of sediment, (*b*) its transportation to and through, or deposition in, the reservoir, and (*c*) remedies for reservoir silting. Quantitative study of sedimentation rates may be made from several approaches, none of which are sufficiently developed to permit more than approximate estimates of probable sedimentation rates. Such estimates may be of use to design engineers, who must allow for inevitable depletion of storage, but the real solution to the problem, both from the technical and social viewpoints, seems to lie in the prevention of sedimentation, and conservation of the available storage to the greatest extent possible.

Introduction

Studies by the Soil Conservation Service (SCS) of the U. S. Department of Agriculture (1)[2] show that, in the silt-carrying streams of the United States, the probable life expectancy of existing reservoirs is dangerously short. Detailed surveys of sediment volumes have been made in about 1% of all reservoirs in the United States. Assuming that these constitute a representative sample, that the long-term silting rate remains uniform, and that a reduction of 80% in the capacity of a reservoir terminates its useful life, these studies indicate that about 64% of all reservoirs have useful lives of less than 100 years, and only about 15% serve for more than 200 years.

Although it is a primary problem in badly eroding areas, silting of reservoirs is not the only manifestation of the silt problem. Among other aspects are sedimentation of navigable channels and harbors; aggradation of flood channels, causing increased frequency of flooding; silting of arable lands and destruction of fertility; increased cost of operating irrigation systems, due to the necessity of frequent dredging or provision of desilting works; increased cost of water

Note.—Written comments are invited for immediate publication; to insure publication the last discussion should be submitted by **November 1, 1943.**

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[2] Numerals in parentheses, thus: (1), refer to corresponding items in the Bibliography (see Appendix II).

supply treatment; damage to mechanical equipment by wear of gates, valves, turbines, etc. Some beneficial effects of sedimentation are decreased seepage losses from reservoirs and canals; decreased vegetation and algae growth due to absorption of sunlight; and fertilization of lands overflowed, when the silt has been produced by sheet erosion rather than by bank or gully erosion.

The factors involved in the production, transportation, and deposition of sediment are many and devious. Approach to the problems has been both empirical and rational: The former has generally seemed productive of the earliest applicable results, but often lacks a sound theoretical basis; and the latter seems more promising of establishing basic principles, but only a beginning has been made. Remedies for reservoir silting are costly and have been neglected, because of additional storage that ordinarily could be developed more cheaply, or because it was impracticable to evaluate the forces involved.

Origin and Nature of Silt

The problem of reservoir sedimentation and its solution are inextricably united with that of erosion. In regions where erosion is negligible, silting of reservoirs is generally not serious; and, where erosion is normally excessive, or has been accelerated by man's activities, reservoir silting is usually rapid.

The factors influencing the erosion cycle may be placed in two broad classes. "Eroding and transporting forces" include rate of runoff, turbulence of flow, fluid shear at boundaries and in eddies, fluid and particle impacts, surface and channel slopes, roughness, and sediment concentration. "Resisting and depositing forces" include gravity, adhesion, protective cover, impermeability, decrease in turbulence, local stilling, penetrable surface cover, and flocculation. Theoretical attempts to evaluate or relate more than one or two of these factors at a time end in practical confusion. The quantitative, empirical approach by way of field survey or reconnaissance gives probably the best data on erosion for engineering usage.

The major factors affecting erosion by water on land surfaces are precipitation and temperature. The impact of large raindrops is believed a significant fraction of the forces causing sheet erosion; the resulting overland flow is a vehicle for the loosened particles of soil. Changes of the overland sheet flow from laminar (streamline) conditions to turbulent flow result in additional erosion. The dynamic action of water in channels adds to the eroded burden. A drop in temperature to or below the freezing point may halt erosion temporarily, since frozen ground is more resistant than unfrozen, but the freezing and thawing cycle itself is a major force in comminuting large rocks and soil particles.

Origin of Sediment.—All sediment in streams originates in sheet erosion or in the various types of bank erosion. Robert Horton, M. Am. Soc. C. E., states that, of the total erosion in the evolution of a humid drainage basin, probably 1% was initially bank erosion, and 99%, sheet erosion (2). Studies of the SCS in the Piedmont (3) suggest that existing deposits within the flood plain of a stream are now often more important as immediate sources of sediment than concurrent sheet erosion, and it is evident in some cases elsewhere that bank erosion predominates over sheet erosion. Principles developed by the SCS indicate that gullying is more important than sheet erosion as a cause

of harmful stream and valley sedimentation; that stream-bank erosion is an important factor in sediment production; and that valley trenching produces large amounts of fine sediment and facilitates downstream transportation.

The watershed characteristics most patently affecting sediment production, and hence the rate of sediment delivery to a reservoir are (4): Degree of erosion; relative extent and distribution of kinds of erosion (sheet, bank, gully, highway, etc.); soil type (size of particles); amount of runoff and character of discharge; and topographic form (colluvial and alluvial deposits).

Definition of Silt.—The erosion process ultimately reduces rock to the finest possible physical particles. The material that fills streams, valleys, and reservoirs actually varies from large boulders to grains just larger than colloidal size. In its widest application, the word "silt" applies to this entire range of sizes; in its more limited meaning, "silt" is a range of particle sizes between that of clay and that of fine sand. Only arbitrary definitions of size can be made, as shown (5) in Table 1.

TABLE 1.—SOIL SIZE CLASSIFICATION[a] (DIAMETERS IN MILLIMETERS)

Class	A.S.T.M. (1)	U.S.D.A. (2)	M.I.T. (3)	Park Service (4)
Sand:				
Coarse	2.0–0.25	1.0–0.50	2.0–0.60	6.0–2.0
Medium	....	0.5–0.25	0.60–0.20	2.0–0.6
Fine	0.25–0.05	0.25–0.10	0.20–0.06	0.6–0.2
Very fine	....	0.10–0.05	....	....
Silt:			....	
Coarse			0.06–0.02	
Medium	0.05–0.005 (Coarse–Fine)	0.05–0.005	0.02–0.006	0.2–0.006 (Coarse–Fine)
Fine			0.006–0.002	
Clay	<0.005	<0.005	<0.002	....
Dust	....	....	....	0.006–0.0002
Colloids	....	....	<0.0002	<0.0002

[a] In Col. 1 "A.S.T.M." refers to classification D422–35T of the American Society for Testing Materials; in Col. 2 "U.S.D.A." refers to the Bureau of Soils, U. S. Department of Agriculture; in Col. 3 "M.I.T." refers to the Massachusetts Institute of Technology; and the last column contains the modified classification of the National Park Service, U. S. Department of the Interior.

The size of the silt particle affects the degree of concentration of a suspension for given conditions of quiescence or turbulence. When the vertical components of turbulence are sufficiently strong, no settling takes place. Research (6) indicates that for each reach of each stream, a "dividing grain size" exists between material which moves as bed load and material which moves in near-permanent suspension. Table 2 indicates the rate of settlement (7) of a particle in still water at 50° F.

Colloidal particles, which include the "fine clay" of Table 2, will not settle out in any practical period of time, due to the effect of like-electronic charges on the particles. Under certain conditions of salt content and temperature, fine silts tend to flocculate and have higher rates of settlement than are shown in Table 2. The existence of graded suspensions results also in greater rates of deposition of the finer materials than when the latter are isolated, as the coarser particles appear to drag down the finer. The presence of organic matter slows up the deposition process, because its weight is more nearly that

of water. Coagulation, or the production of flocculation by chemical means, as in water supply reservoirs and treatment tanks, hastens settling. Flocculation is aided by calcium and magnesium salts, and counteracted or prevented by sodium or potassium salts; obviously, the geologic composition of a watershed may favor a flocculated silt in some instances.

TABLE 2.—RATE OF SEDIMENTATION[a] IN STILL WATER AT 50° F

LINE	COARSE SAND		FINE SAND				SILT	COARSE CLAY	FINE CLAY
1	1	0.20	0.10	0.06	0.04	0.02	0.01	0.001	0.0001
2	100	21	8	3.8	2.1	0.6	0.15	0.0015	0.000015

[a] Line 1 contains the diameters of particles, in millimeters; and, Line 2 contains the rate of settlement, in millimeters per second.

Silt deposited in a reservoir varies greatly in weight and volume, depending on its source, the depth of deposition, and the degree of submersion or exposure. The percentage of voids in a deposited silt sample appears to be the most important criterion of silt weight and volume. Assuming an average specific gravity of 2.6, which varies little in any case, J. C. Stevens, M. Am. Soc. C. E. (8), presents the values for silt deposits shown in Table 3.

TABLE 3.—WEIGHT OF SILT, IN POUNDS PER CUBIC FOOT

Weight	PERCENTAGE OF VOIDS:									
	0	20	30	40	47.8	50	60	67.5	70	80
Dry...............	163	131	114	98	85	81	65	53	49	33
Saturated..........	163	144	133	123	115	113	102	95	93	83

Silt from exposed beds in Elephant Butte Reservoir (Rio Grande) has been found to weigh from 97 to 124 lb per cu ft, and the dry weight of samples from various points along the Colorado River has varied from 37 to 102 lb per cu ft. Samuel Fortier and Harry F. Blaney, M. Am. Soc. C. E. (9), recommend for the Lower Colorado a specific weight of 62.5 lb per cu ft for suspended silt, and 85 lb per cu ft for deposits in reservoirs.

The space occupied by silt after deposition in a reservoir changes with time and with the method of operation of the reservoir. Superimposed sediment compacts the lower deposits, as also do exposure and drying. The degree of shrinkage upon drying is dependent upon the relative sand and clay contents of the deposit. Sandy deposits shrink relatively little, but fine, uniform clay deposits are subject to considerable shrinkage. Deltaic deposits, worked over by wave action, may be well graded and quite dense.

Borings in exposed silt deposits in storage basins indicate that a definite water table exists commensurate with the water level of the reservoir; deposits below the water table are flocculent; and deposits do not shut off spring-water inflow on the beds of reservoirs.

NOTATION

The letter symbols used in this paper are defined when they first appear and are assembled for convenience of reference in Appendix I.

TRANSPORTATION OF SEDIMENT TO THE RESERVOIR

The sediment produced by erosion finds its way to the reservoir by movement in suspension, and movement as bed load. The former accounts for the transport of most of the finer sediment, whereas the coarser sands and gravels, and boulders, are moved by rolling along the bed of the stream. A third, or intermediate, stage of transportation exists—that of "saltation"—whereby movement becomes a series of steps or jumps; but this process is more predominant in littoral transportation. Hunter Rouse, M. Am. Soc. C. E., pertinently states that theoretically there is no fixed dividing line between the two primary modes of movement, but that limited knowledge of the correlation between the two precludes any but an independent treatment for the present (10). As previously stated, however, there now appears to be at least an empirical and unique division between grain sizes carried by each mode of transportation in each stream reach.

One of the earliest and best known attempts to evaluate the effect of flowing water on sediment was the formula developed by R. G. Kennedy for the Bari Doab Canal in the Punjab, India, about 1900. This formula is of the general form:

$$V_c = C_k \, (y_d)^x \qquad (1)$$

in which V_c = a "critical" velocity that will neither scour nor deposit material; C_k = an empirical coefficient, depending on the kind of silt; y_d = depth of water in channel; and x = an exponent, obtained from plotting observed data on logarithmic paper.

Eq. 1 has been used widely for canal design, with more or less successful results, but the wide range of values for C_k and x indicates that almost every application is unique. The coefficient, C_k, has been found to vary (11) from 0.38 to 1.83, and the exponent, x, from 0.44 to 0.73. There appears to be no relation, inverse or direct, between C_k and x. It may be concluded, therefore, that the range of velocities in the non-eroding, non-silting state is either little dependent on the depth, or highly dependent on the nature of the silt. Other formulas have since been proposed, but these also require the evaluation of empirical constants. At best, the available formulas are but guides to judgment. No general or certain analytical method is available for the determination of the "critical velocity," as defined by Mr. Kennedy.

Suspended Load.—After material has been taken into suspension, turbulence theory offers an explanation of subsequent sediment transportation. The theory of fluid turbulence also suggests the principle of vertical distribution of sediment in a stream, and therefore permits more accurate estimates of silt load from sampling observations.

Turbulence in a fluid represents the transfer of energy or momentum in random manner from one particle of the fluid to another. If such a fluid con-

tains silt, some of the energy is transferred from fluid to silt, and from silt particle to silt particle. In maintaining suspension, the net transfer of energy is the resultant of all upward forces against the forces of gravity and other deposition factors. G. I. Taylor defines turbulence as "* * * an irregular motion which in general makes its appearance in fluids, gaseous or liquid, when they flow past solid surfaces, or even when neighboring streams of the same fluid flow past or over one another" (12). Theodor von Kármán, M. Am. Soc. C. E. (12*a*), in substance defines the scientific term turbulence as implying irregular fluctuations, governed by laws of some statistical equilibrium between the random forces of turbulence and the force of gravity.

Various research workers have developed formulas to express the degree of turbulence, the turbulent energy, and its mode of transfer to other fluid particles and to the suspension of sediment. The formulas require a number of empirical constants, but the science rests on sound physical and rational foundations, a source of criticism against the purely empirical attack.

The present theory of fluid mechanics assumes a completely homogeneous and fluid substance. As soon as sediment is introduced into a stream, conditions depart from the ideal state, since both sediment and fluid are now moving, not only with respect to the boundaries of the fluid, but with respect to each other. Professor Rouse (10) as well as Morrough P. O'Brien, M. Am. Soc. C. E. (13), have reviewed the relation of turbulent flow to sediment transportation in some detail, and reference to their work is suggested.

Research has established fairly well that the concentration is dependent on sediment grain size, water discharge, and the degree of turbulence, at a particular instant. The total quantity of sediment is not necessarily related directly to discharge at all times, because of seasonal variations in the supply and source of sediment, and distribution of rainfall and runoff from the watershed, so that a measurement of the sediment load for a given discharge does not indicate the amount which may be carried by an equal discharge at another time. E. W. Lane, M. Am. Soc. C. E., and A. A. Kalinske, Assoc. M. Am. Soc. C. E. (14), have developed analytical expressions, based on the theory of the mechanics of fluid turbulence, for the distribution of sediment concentration in a vertical, and for the total suspended load in a unit width of a "wide" stream. Simplifying assumptions are that: The stream is "wide"; the kinematic viscosity is constant throughout the depth; the turbulence is due to an unconsolidated stream bed; and the sediment composition by sizes is known. The formulas permit estimating sediment distribution and load in a unit section from a single silt and velocity measurement in the section. The formula for distribution of sediment concentration in a vertical is:

$$N = N_a \, e^{-15t(z-y_a)} \quad \text{(2a)}$$

and that for total sediment load in the unit section is:

$$W = V_m \, y_d \, P \, N_a \, e^{15t y_a} \quad \text{(2b)}$$

In Eqs. 2, N = sediment concentration at any point in the vertical, at a distance, y, above the bottom; N_a = measured sediment concentration in the

vertical, at a distance, y_a, above the bottom; e = base of Napierian logarithms (2.718); t = a dimensionless parameter $= \frac{c}{\sqrt{g\, y_d\, S}}$; c = mean velocity of fall of sediment within a selected size range; g = acceleration of gravity; S = slope of energy gradient of the stream at the time of measurement; z = relative depth of point, above the bottom $= \frac{y}{y_d}$; y_a = distance of sampling point above bottom; y = distance above bottom of the point for which the concentration is desired; V_m = mean velocity in the vertical (or in the unit section); P = ratio of mean concentration in the vertical to the concentration at the bottom; n = Manning's roughness coefficient; and W = total weight of sediment within a given size range in a unit width of stream.

Eq. 2*a*, with z plotted arithmetically against N on semilogarithmic paper, results in a straight-line curve having a slope of $-15\,t$. In 1941 a report of the U. S. Engineer Office (15) showed the slope as $-16\,t$, and presented a plot of P versus t for three values, 0.01, 0.02, and 0.03, of the relative roughness. Eq. 2*a* may therefore be solved graphically if N_a is known. Eq. 2*b* represents the integration of Eq. 2*a*, after inserting a term for velocity. The term, P, is approximated by the following expression (15), and must be evaluated for the proper conditions:

$$P = \left\{ \left[1 + 9.50 \frac{n}{(y_d)^{1/6}} \right] \int_0^1 e^{-15tz}\, dz \right\} + \left\{ 9.50 \frac{n}{(y_d)^{1/6}} \int_0^1 e^{-15tz} \log_e z\, dz \right\} \qquad (3)$$

Eqs. 2 and 3 permit more accurate estimates of suspended sediment than by averaging a few measurements at arbitrary sampling points. However, very fine suspended sediment is so evenly distributed through the depth that a single measurement in a vertical or unit section suffices for silts and finer sediments as defined in Tables 1 and 2. It is dependent on the individual investigator whether the increased accuracy warrants the considerable increased time and expense involved in making the necessary measurements and analyses.

If a reasonably long record of suspended sediment measurements is available, a correlation between suspended sediment and discharge may be observed by plotting the data on logarithmic paper. A straight line usually fits the data as well as any other curve, and Frank B. Campbell, M. Am. Soc. C. E., and H. A. Bauder (16) call the relation a "silt-rating curve." However, the scatter of points is deceptive on logarithmic paper, and, for reasons previously stated, an estimate therefrom of silt load in an unsampled flow may be considerably in error. Nevertheless, the curve may be used as an indication of the probable average relation and, hence, it is sufficiently accurate for use in estimating the average annual silt quantities delivered to a reservoir. Messrs. Campbell and Bauder made such use of data for estimating the probable rates of silting for the reservoir above Denison Dam on the Red River, Texas. The equation of the curve is of the general form:

$$G_s = C_s\, Q^x \qquad (4)$$

in which G_s = suspended sediment, in tons per second; C_s = an empirical coefficient in suspended sediment rating curve; Q = water discharge, in cu ft per sec; and x = an exponent (the slope of the logarithmic plat).

Fig. 1 shows such "silt-rating curves" for the Black, Cuyahoga, and Grand rivers, in Ohio, based on several years of daily measurements taken in 1902–1904 (17). The data are admittedly of limited accuracy. The samples were obtained near the mouths of the streams by submerging rapidly a wide-mouthed 8-oz bottle at midstream to about 10 ft; and the discharges, based on a metered rating curve, are the averages of twice-daily readings. The silt measurements for the Denison River, on the other hand, appear to be of a higher order of

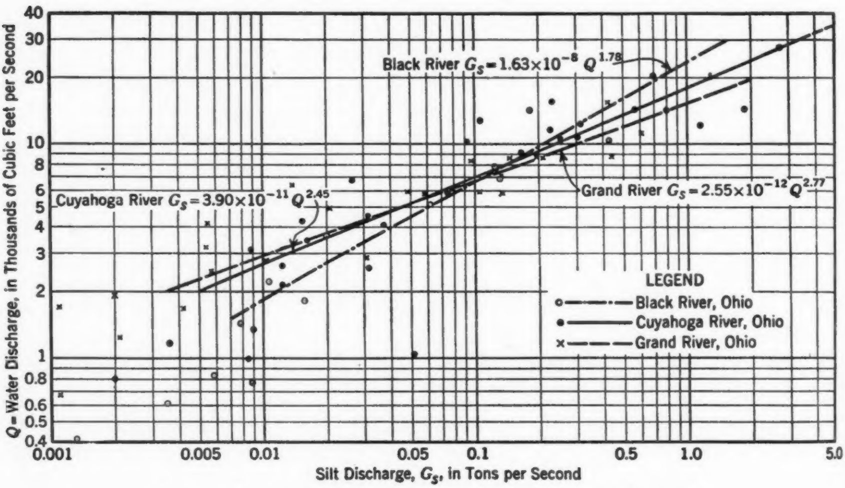

FIG. 1.—TYPICAL SILT RATING CURVES (SEE EQ. 4 AND TABLE 4)

accuracy, in that they represent averages of samples taken at 0.6 depth at 1/6, 3/6, and 5/6 of the river width, with the center sample given a weight of 2. The scatter of points above and below a certain range suggests the existence of different correlations. The lower values are of relatively little importance compared to the total annual silt load; the higher values accompany only extreme floods of comparatively rare frequencies and, although of weighty magnitude, also represent but a small part of the average annual load. The equations of the three curves of Fig. 1 are compared in Table 4 with that derived by Messrs. Campbell and Bauder, for the Red River (16).

The average annual suspended silt load can be computed by a combination of the "silt rating curve" and the discharge-frequency curve. It is first necessary to compute the ratio of silt load to corresponding discharge for a sufficient range of points, and then to plot the ratio $\frac{G_s}{Q}$ against the percentage chance of occurrence for the proper discharge, on arithmetic paper. The area under the curve represents the average annual suspended silt load per cubic foot per second of water discharge and is reduced to total average annual load by multi-

plying by the total average annual volume of discharge. Based on the equation in Item 3, Table 4, and on discharge-frequency relations adopted for an unpublished Engineer Department report, the area under the curve in Fig. 2 indicates that the probable average annual silt load of the Cuyahoga River at the mouth is about 0.0425 lb per cu ft of water, or, for a mean annual discharge

TABLE 4.—Typical "Silt Rating Curves"

No.	Geographical region	River	Place	Curve	Limits of Discharge (Cu Ft Per Sec)	
					Lower	Upper
1	Southwest	Red[a]	Denison, Tex.	$G_s = 9.5 \times 10^{-9} Q^{2.036}$	1,000	20,000
2	Great Lakes	Black	Lorain, Ohio[b]	$G_s = 1.63 \times 10^{-8} Q^{1.78}$	2,000	30,000
3	Great Lakes	Cuyahoga	Cleveland, Ohio[b]	$G_s = 3.90 \times 10^{-11} Q^{2.45}$	2,000	30,000
4	Great Lakes	Grand	Fairport, Ohio[b]	$G_s = 2.55 \times 10^{-12} Q^{2.77}$	1,500	25,000

[a] Reported by F. B. Campbell and H. A. Bauder (16). [b] At the mouth of the river.

of 794 cu ft per sec, about 532,000 tons per year. U. S. Engineer Department records show that about 650,000 cu yd of silt (or about 526,000 tons, assuming a concentration of 60 lb of dry sediment per cubic foot of deposit in place) are removed annually in maintenance dredging of the navigable areas of Cleveland Harbor, most of which comes from the Cuyahoga watershed. No reservoirs

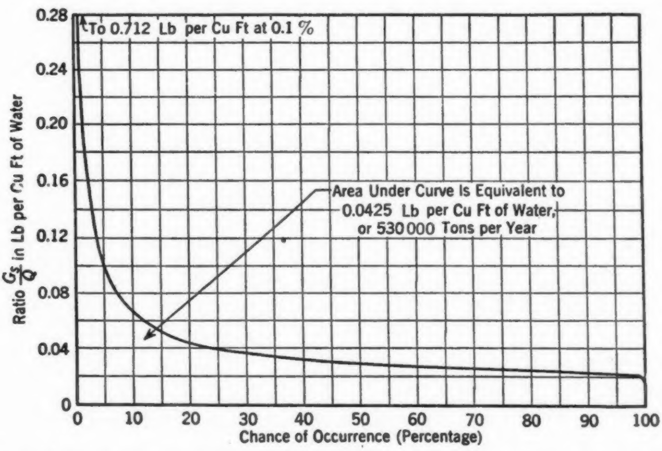

Fig. 2.—Graphical Determination of Probable Annual Suspended Silt Load of Cuyahoga River, Ohio, at the Mouth

of adequate size exist on the Black, Cuyahoga, or Grand watersheds, to permit a comparison with actual rates of reservoir silting, and similar data for other areas are not available. Rates of silting in harbors are not directly comparable with those in reservoirs, and it is unlikely that the total load of the Cuyahoga is deposited in Cleveland Harbor. Some undetermined amount reaches the open lake. Also, part of the deposition in Cleveland Harbor is due to bed load

from the river, subaqueous erosion from undredged areas, and possibly a small volume carried into the harbor by littoral forces. Considering the accuracy of the data in this example, the method seems reasonably applicable in determining the average annual suspended silt load of a stream.

Bed Load.—The problem of evaluating the bed load of the stream remains. Bed load appears to move first in a uniformly distributed layer, which begins when the tractive force reaches a certain value, and then in riffles which travel downstream, beginning at some larger critical tractive force (18). Bed-load sediment consists of those particle sizes which are too large for movement in suspension by the available suspending forces. The line of demarcation, as noted under the heading "Origin and Nature of Silt," is flexible, and, to the writer, it appears probable that this "limiting size" might have to be evaluated for various ranges of discharge, as well as for individual reaches in a stream. Investigation of the phenomenon indicates that bed load, unlike suspended load, is directly related to the discharge at all times, because the sediment supply for the bed-load forces is nearly always available, except on debris-clear rock-lined channels.

A greater amount of work appears to have been done on developing formulas for the amount of bed load than for suspended load. Experiments on flume traction by G. K. Gilbert (19) pointed the way for other workers. Two typical formulas are the Schoklitsch and the M.I.T. formulas. Investigations for the design of the All-American Canal (20) indicated that "the Schoklitsch bed-load formula is still a reliable instrument for the hydraulic engineer * * *." This formula, expressed in English units, is:

$$G_b = \frac{86.7}{(D_g)^{1/2}} S^{3/2} (Q_i - b\, q_o) \qquad (5a)$$

and the M.I.T. formula (18) may be expressed as follows:

$$G_b = C_b\, S^{x+1} (Q_i - b\, q_o) \qquad (5b)$$

In Eqs. 5, G_b = total bed load, in pounds per second; D_g = effective grain diameter, in inches; S = slope of the energy gradient; Q_i = total instantaneous discharge, in cubic feet per second; b = width of river, in feet; q_o = critical discharge, in cubic feet per second per foot width at which movement begins $\left(= \frac{0.00532\, D_g}{S^{4/3}}, \text{ in Eq. } 5a\right)$; and C_b and x = constants in Eq. 5*b*, depending on the specific gravity and mechanical composition of the bed material. The application of Eq. 5*b* is obscure since C_b and x "are yet to be related to sand mixtures so as to be determined directly from mechanical analysis" (18).

An approximate solution for bed load by the Schoklitsch formula can be made by determining or assuming a single representative grain size of the bed-load sediment, and mean values of the slope and discharge. However, it is obvious that the "critical" grain size varies within a given reach of a stream with different flows, so that only very limited accuracy can be expected. An estimate of the finest bed-load grain size for particular discharge and slope conditions may be made by analyzing the bed-surface material immediately

after a flood. Assuming a mean grain size of 0.04 in. (about 1 mm) for the Cuyahoga River bed sediment near the mouth, an average slope of 1.0 ft per mile, and a mean annual discharge of 794 cu ft per sec, the total annual bed-load movement would be about 9,000 tons per year. Accurate computations of bed load are not possible because of variations of bed material from time to time, shifting of the effective width of bed that is actually scouring, and the necessary assumptions that must be made.

The practical and successful application of available formulas depends on expressing the nature of a mixture of grain sizes in some equivalent mean diameter, and evaluating the initial transporting force. No simple mathematical means of doing so are available. The theoretical functions presented by H. A. Einstein, Assoc. M. Am. Soc. C. E. (21), eliminate the idea of a "critical" force or state, and include the characteristics of the particles, but, although these functions describe experimental data, they are quite complex, and do not readily permit estimates of bed load for the varied conditions met in the field.

The Action of Sediment in Reservoirs

Some streams carry appreciable quantities of sediment only during floods, whereas others are burdened constantly with large silt loads. Whether the major part of silt damage is done intermittently or continually, the process of deposition is essentially the same. Some phenomena, however, are evident in reservoirs that are not so obvious in natural lakes or in the sea. Deposition of sediment results from the removal or dissipation of the transporting forces. The heavier silt sizes are naturally the first to be deposited, with the smaller sizes being carried farther out into the reservoir or not being deposited therein at all. The late O. A. Faris, M. Am. Soc. C. E. (22), partly describes the process thus:

> "Suspended silt settles to the reservoir bottom soon after entering the slack water and, having a greater specific gravity than water, flows, in the form of liquid mud, down the slopes into depressions and along the main channel until blocked by the dam. Owing to its greater density, silt-charged water entering a reservoir partly filled with clear water does not mingle with the clear, but forces it downstream toward the dam. No suspended silt is carried through the reservoir and over the spillway until all of the clear water has been discharged."

Deltas.—However, the sedimentary load of a stream is actually reduced considerably by valley aggradation above the reservoir. The remaining coarse-grained sediments, whether carried in suspension or as bottom load, tend to form deltas at the points of inflow. These deposits may be greatly altered from the ideal delta form by variations in reservoir level. Where the reservoir level is fairly constant, the characteristic bottom-set, fore-set, and top-set beds of a conventional delta are present, and the delta is subject to subsequent entrenchment by the stream with redeposition of the material farther out in the reservoir. If coarse sediments are lacking, no delta may be formed. The presence of deltaic deposits in a reservoir too frequently distracts attention from the unseen bottom deposits.

Bottom Deposits.—The fine-grained sediments carried by a stream into a reservoir spread over the bottom, and tend to concentrate in the deeper areas, especially immediately above the dam. This action is aided by the flow of the heavier turbid water along the thalweg beneath the lighter clearer waters of the lake. When the stilling effect of a large storage basin is available to an incoming sediment-laden stream, practically all of the sediment may be deposited in the reservoir cavity, and the outflow may be almost crystal clear. In such cases, an estimate of the probable average silt load of the tributary streams will indicate the rate of silting of the reservoir. Carl B. Brown of the SCS states that most of the reservoirs surveyed thus far trap between 70% and almost 100% of incoming sediment (23).

Effect of Reservoir Shape.—The distribution of sediment in a reservoir depends on the shape of the basin. If the reservoir is regular in shape, deposits from suspension will be distributed quite uniformly along its axis, decreasing in depth with distance above the dam. If the reservoir is irregular, there may be marked irregularity in the depths of the bottom-set beds. This is illustrated in the case of Elephant Butte Reservoir, where a section, known as "The Narrows," exists from about mile 15 to mile 19 above the dam. Profiles of silt deposits (1*a*) show that the silt surface here is almost horizontal, instead of following the grade of the original valley floor. However, the upper end of Elephant Butte is partly affected by seasonal drawdown.

Density Flows.—Considerable sediment is distributed to the deepest parts of a reservoir by the denser silt-laden water flowing beneath the lighter desilted water. Such flows sometimes dispense their load by diffusion and mixing or by settlement, thus losing their identity. However, it has been observed frequently in Lake Mead (above Boulder Dam), at Elephant Butte Reservoir, and in various reservoirs in Texas and in the Piedmont, that muddy underflows often extend to the outlet through a superficially clear lake. Ultimate understanding of the principles of density flows may lead to some control of reservoir silting. If the flow is allowed to stagnate, the silt will be deposited in the reservoir; if such flows could be induced and controlled, much of the sediment entering a reservoir could be voided before deposition. However, density flows are sometimes of quite low silt content, with the greater part of the difference in density from the reservoir water due to salt content or temperature.

Study of density flows in the past was generally neglected, because such flows are mostly invisible or hidden in the depths of the reservoir. However, two visual characteristics of underflow in reservoirs are the sharp line of demarcation near the head of the reservoir where the muddy water plunges below the clear water, and the localized zone of floating debris created at the upper end of the reservoir by a return current induced at the surface.

Density Flows in Lake Mead.—With reference to three observed occurrences of silty discharges at Lake Mead during 1935, Nathan C. Grover, M. Am. Soc. C. E., and Charles S. Howard (24) state that the flows apparently passed entirely through the reservoir, "essentially unmixed," and ascribe the phenomenon to the greater specific gravity of the incoming silt-laden water with respect to the clearer water at the surface of the lake. They estimate that these three silt flows carried some 6,000,000 tons of sediment, or about 2.5% of the average

annual silt load brought to Lake Mead. Although measurements were made at Willow Beach about 10 miles below the dam, these quantities apparently do not include any materials scoured from the river channel below the dam. Mr. Faris (24*a*) disagrees with this theory and suggests that such discharge is the result "* * * of disturbance of silt which was virtually in place; that is, was no longer in suspension * * *." He implies that operation of the gates disturbed the equilibrium of this almost-liquid deposit and caused it to flow out of the reservoir. Ivan E. Houk, M. Am. Soc. C. E. (24*b*), suggests that lake temperature distribution is an important factor in density flows. In the case of a deep lake, movement may occur at some intermediate level rather than along the bottom of the lake, depending on the density of the silt flow. In order that silt may appear at the outlet of a lake, Mr. Houk believes that a relatively large inflow must continue until the silty flow has had time to reach the gates. On the other hand, Paul A. Jones, M. Am. Soc. C. E. (24*c*), suggests that the density flows of 1935 were due to the initial submergence and resulting erosion of unstable existing deposits as the reservoir filled. Hugh Stevens Bell (25) evaluates knowledge gained at Boulder Dam since 1935, and estimates that silt-laden density flows in Lake Mead carry about 24% of all sediment deposited in the lake.

Formation and Dispersion.—Experiments conducted at the California Institute of Technology, in Pasadena (26), show that "* * * density currents are not delicate or easily destroyed, but are difficult to prevent and more difficult to destroy." The loss of energy as the stream enters the reservoir is not generally sufficient to prevent the formation of a density current. Once the current has formed, only mixing across the "interface" can disperse it. The particles in suspension in the flow are believed to furnish most of the "driving force" down the slope of the reservoir bed, by virtue of their relatively greater specific gravity over that of water, so that the silt conveys the water rather than the water carrying the silt. Observation verifies the hardiness of density flows in the laboratory and in natural lakes and reservoirs. Density currents are known to have flowed for 100 miles through Lake Mead, and for 35 miles through Elephant Butte Reservoir. After storms on Lake Erie, the Niagara River carries considerable sediment into Lake Ontario. At the mouth, part of the river current flows out into the lake and part turns almost 90° to the east, flowing as a distinct stream at various distances offshore, depending on wind and wave conditions.

Mr. Einstein (27) infers that density flows may be considered laminar, and that dispersion, or mixing across the interface, will occur only when the flow is "unstable" and turbulence is caused. The turbulence must not be so great as to overcome the depth of the density current and break through the interface. He states that "* * * if the depth and concentration of the suspension are sufficiently high, and the sediment is flocculated, even violent turbulence will not reach the interface from the bed." Existing knowledge does not indicate the quantitative influence of dissolved matter, such as salts, and of temperature. No methods are known for quantitative analysis of density flows. Mr. Einstein states that "* * * not enough is known of turbulence structure to permit even estimates * * *." Additional field data are especially needed to sub-

stantiate the results of laboratory study. A fuller understanding of the phenomena involved may well lead to a utilization of density flows for controlling the rate of reservoir sedimentation to some extent.

Silting of Flood Control Reservoirs.—Few observations of silting have been made in flood control reservoirs. In general, however, silting therein appears to be a less serious problem than in storage basins. Due to drying and shrinking of silt beds in flood control reservoirs which are emptied after each flood, the silt consolidates and does not regain its original volume when submerged. Studies made on the Germantown and Englewood detention basins of the Miami Conservancy District by E. W. Lane and J. C. Kennedy (28) suggest that retarding basins do not silt up quickly, since a large part of silty and clayey sediment passes through without being deposited; that the proportion of clay to sand deposited increases with the time of detention; that recreational or other permanent storage pools within a retarding basin may be seriously silted within a short time; and that, unless the inflow contains high sediment concentrations, or the detention period is unusually long, sedimentary deposits are unlikely to become serious for many years. No evidence is available, but it seems improbable that density flows as previously described could exist in purely detention-type reservoirs, because of the more or less thorough mixing of the inflow.

Estimating the Rate of Reservoir Sedimentation

Methods of estimating the rate of sedimentation in a proposed reservoir are desirable, technically and economically, and may be approached at present from several angles. Comparison may be made of sedimentation rates in other reservoirs exposed to similar conditions; or an estimate may be made of the probable quantities of silt that will be delivered to and trapped by the reservoir. Unless specific remedies are to be undertaken to prevent sediment production and delivery, it is necessary to make proper allowance for silting, but even the fullest possible measure of control of sediment production will not eliminate partial depletion of storage.

Effectiveness of Reservoirs as Sediment Traps.—The quantity of sediment deposited in a reservoir is dependent on the extent to which deposition can take place. For complete deposition of the silt load, the capacity of the reservoir would have to exceed the volume of the incoming flood, and would have to retain the flood sufficiently long to permit all of the sediment to settle out. A criterion of reservoir effectiveness as a trap is the ratio of reservoir capacity to the drainage area above the dam. It appears that reservoirs having more than 15 acre-ft of capacity per square mile of drainage area (about 0.023-ft depth on the watershed) are comparable for rates of silting, whereas those of smaller capacity are not (29). The latter reservoirs either are filled by bed load, with little retention of flood waters, or a large part of the deposits are vented by operating the gates at the dam.

"Regional Indexes."—The SCS has given considerable study to sediment surveys in existing reservoirs, and the data thus obtained are presented in their official reports (1). Fig. 3 presents graphically the average sedimentation rates for various reservoirs reported by the Service (1*b*). The writer has drawn

envelope curves for storage reservoirs in the four geographic regions noted in Fig. 3 and in Table 5. The envelope curves have been drawn in "by eye"; the two points above curve I were neglected because of the small storage involved

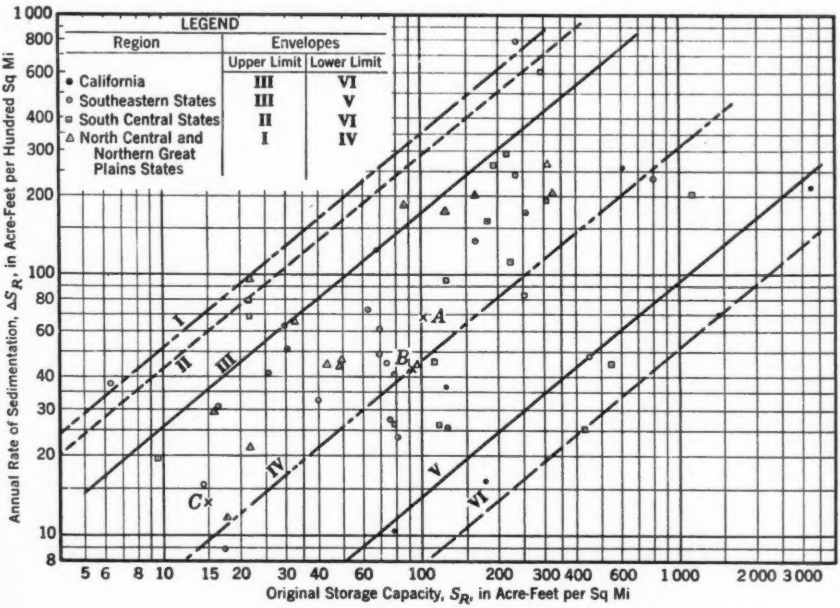

FIG. 3.—SEDIMENTATION RATES IN EXISTING STORAGE RESERVOIRS

or the short time elapsing between surveys. The equation is of the general form:

$$\Delta S_R = I\ (S_R)^x \quad \text{(6)}$$

in which ΔS_R = annual silting rate or depletion of storage, in acre-feet per 100

TABLE 5.—"REGIONAL INDEXES" (EQ. 6) OF STORAGE RESERVOIR SEDIMENTATION

Region (data are from reservoirs in the states indicated)	UPPER LIMIT		LOWER LIMIT	
	Envelope	I	Envelope	I
Southern Pacific Coast				
California	III	3.75	VI	0.167
Southeast				
Alabama, Georgia, Maryland, North and South Carolina, and Virginia	III	3.75	V	0.307
South Central				
Arkansas, Missouri, Oklahoma, and Texas	II	6.24	VI	0.167
North Central (Including the Northern Great Plains)				
Illinois, Kansas, Montana, Nebraska, and South Dakota	I	7.56	IV	1.01

sq miles of drainage area; I = coefficient, herein termed the "regional index"; S_R = original storage, in acre-feet per square mile of drainage area; x = 0.83 = the graphic slope of the plat on logarithmic paper.

The data of Fig. 3 are from large geographic regions, differing considerably in soil, erosion, meteorologic, and hydrologic conditions. It should be expected, therefore, that distinct indexes could be determined for each region; but, as the envelope curves of Fig. 3 indicate, there is considerable overlapping, so that only the probable range of sedimentation rates can be estimated. The envelope curves are parabolic in form, and a logarithmic slope of $x = 0.83$ seems to fit the data fairly well. The coefficients, which might be termed "regional indexes," have a wide range for a given region. Of course, data obtained in the future may either spread the envelopes, or narrow them; the latter seems probable in that reservoirs surveyed thus far are more typical of severe rather than of average conditions. It is of interest to note that Elephant Butte and San Carlos reservoirs in the Southwest, and Black Canyon Reservoir in the Northwest (points A, B, and C, respectively, in Fig. 3) are the only examples from these two regions; and yet they plot reasonably near the geometric mean of the envelopes for all the stated regions except the North Central and Northern Great Plains.

It would be desirable if the data indicated narrower ranges of sedimentation rates within a given region. Isolation of such factors as annual rainfall and runoff, rate of sediment production (erosion), variations in operation of different reservoirs, etc., would probably produce this result, but available data are insufficient for this purpose. However, the data accumulated by the SCS, presented in part in Fig. 3, are invaluable in that

> "* * * the principle of the limited range of debris production makes it possible henceforth to take this factor into account in design, and to predict from regional indices of sediment production developed through widespread reservoir surveys and sediment load measurements, the order of magnitude of the silting rate, and the relative need for sediment control measures within the limits of design to which a specific reservoir must be limited" (29).

Sediment Trap Efficiency.—In an attempt to narrow the range of sedimentation rates indicated by considering only the rate of silting and volume of reservoirs, as in Fig. 3, G. M. Brune and R. E. Allen, Jun. Am. Soc. C. E., of the SCS studied the effectiveness of a single type of storage pool in trapping sediment (4). The rate of erosion was estimated from erosion surveys or reconnaissances; the rate of silting, from reservoir studies; and the percentage of eroded soil trapped was computed. Data for twenty-five reservoirs in the Ohio Valley region (Indiana, Kentucky, Michigan, Ohio, and Tennessee), plotted logarithmically against storage volume in acre-feet per square mile, produced a "band" of values indicative of the "trap effectiveness" of storage reservoirs. The envelopes and median curve may be closely defined by:

$$100 \left(\frac{\Delta S_R}{E} \right) = C_T \, (S_R)^x \qquad (7)$$

in which: E = sediment produced annually on watershed (annual rate of erosion); and C_T = "trap efficiency" coefficient for the reservoir. For the Ohio Valley region, $x = 0.5$, which may also hold for other regions. The coefficient C_T has a range from 0.91 to 4.34, with a graphic mean of 2.00. In applying to

tentative designs, it would be necessary to select a value of C_T that would properly allow for the contemplated operating conditions, and any abnormal soil, erosion, or reservoir conditions. If the reservoir were to be used primarily for storage, selection of a high value of C_T would be conservative; if an estimate of the desilting efficiency were desired, assumption of a low value of C_T would be on the safe side.

Eq. 7 would give only the probable percentage of eroded sediment that would be caught by a reservoir. The annual rate of depletion of storage may be computed by estimating the rate of erosion on the watershed, for those cases where survey data are available. An assumption must be made of the probable density of silt deposits under proposed conditions. As stated, the SCS has found that storage reservoirs trap "between 70 and almost 100 percent" of the sediment delivered to them (23).

Estimates of sediment delivery to a reservoir may be made also on the basis of suspended silt and bed load carried in by tributary streams, in accordance with procedure and formulas given in a previous section of this paper.

In designing a reservoir, estimates should be made of probable silting rates by all feasible methods. The general lack of data and knowledge concerning the process does not permit very great accuracy, but judgment may reach reasonable conclusions by considering such "regional indexes" as are now available, the effectiveness of vari-sized reservoirs as sediment traps, the rate of erosion on the watershed, and the rate of sediment delivery by tributary streams. These factors all can be measured or estimated. Data already accumulated are of great value, and the continuation of such studies is highly desirable from the practical engineering viewpoint.

Remedies for Reservoir Silting

Authorities generally agree that redemption of lost reservoir capacity is practically impossible, due to the nature of silt, its distribution in a large reservoir, and the usually large quantities present. The most practicable means of avoiding this loss are to prevent the formation of permanent deposits, and to control the rate of sediment production from eroding areas. When this cannot be done, sufficient storage space must be provided to compensate for depletion by silting during a reasonable economic lifetime.

Two general methods in use to remove silt from reservoirs after deposition are mechanical excavation, as practiced in the debris basins of California, and in some small municipal and industrial reservoirs; and removal of silt by sluicing above the dam.

Excavation.—Removal of silt by mechanical excavation involves high maintenance costs. It has been found feasible in California only because of the severe damages caused by debris flows from arid eroded watersheds. The basins are built especially to trap the debris, often comprising as much as 90% of the total flood volume. The sediment is removed from the basin by steam shovel, or other mechanical means, at a cost of about 30¢ per cubic yard. A related problem affecting costs is the disposal of the excavated material, which sometimes must be hauled several miles from the basin (30). The debris

problem in California is unique, and the social and economic aspects differ considerably from the reservoir silting question in other regions. Mechanical removal of silt does not appear to have widespread practicability.

Sluicing.—Silt may be sluiced from a reservoir by the provision of small gates to drain the reservoir when seasonal requirements are low, thereby cutting away the deposited sediment above the dam; or by the provision of large flood gates which would pass the entire silt-laden flood flow, thereby preventing most of the deposition. The first procedure has not been effective generally, except in scouring out the deposits for short distances above the dam. Although used with "considerable success" on the Hambra and Hamiz reservoirs in Algeria, it has had little success with deposited silt in Bhatgurk Reservoir in India, and Zuni Reservoir in the United States (8). The second procedure eliminates the flood control value of the reservoir and merely passes the flood and silt problem to downstream areas. However, it has been found practicable in the irrigation reservoirs of Egypt, where the population of lower areas desires the fertilizing silt brought down from the Upper Nile River.

Use of density flows, previously described, may be considered a possible remedy analogous to sluicing, but the silt carried by such currents must be voided, before the density current comes to rest. Mr. Bell (25) suggests some interesting uses that might be made of density flows, which not only would result in voiding some sediment from the storage cavity, but would permit operating a large reservoir to provide silty water for the repair of leaky canals and the control of vegetation in channels. Knowledge of density flows in reservoirs is insufficient at present to indicate the extent to which they may be made to serve a useful purpose, but further study is warranted in determining remedies for reservoir silting.

Erosion Prevention.—Prevention of erosion, by checking sediment production at the source, appears to be the best remedy for minimizing the silting of a reservoir. It has been estimated that if all possible erosion-control measures were applied to the Colorado River watershed, a possible reduction of 15% to 20% could be made in the present silt load of the river, thereby adding 100 to 200 years to the life of the reservoir system (31).

Erosion-control measures are correctly termed "upstream engineering," as distinguished from "downstream engineering," which involves large dams and reservoirs and channel projects. "Upstream engineering" consists of reforestation, halting of gully growth by check dams and planting, contour plowing, regulation of crop and grazing practices, proper treatment of high embankments and cuts, and stabilization of stream banks.

One phase of reforestation that may also be applied near the reservoir is the planting of proper vegetation screens and the construction of earth barriers on the flats at the head of a reservoir and above the mouths of large tributaries. Such screens reduce the velocity of silt-laden inflows and cause valley deposition before the silt reaches the main cavity of the reservoir. The accidental introduction of tamarisk above Lake McMillan on the Pecos River in New Mexico in 1912 produced a dense screen over the entire upper end of the reservoir and for some 200 miles upstream. The annual rate of sedimentation in the lake

dropped from 13.30 acre-ft per 100 sq miles of drainage area in the period 1910–1915 to 0.98 acre-ft per 100 sq miles in 1925–1932 (1).

Desilting Works.—The desilting works of the All-American Canal at Imperial Dam were designed to desilt 15,000 cu ft per sec of water. The silted water is passed through circular stilling basins, with a maximum velocity of 0.24 ft per sec, and a detention period of 14 min. Only material larger than 300-mesh is removed. The deposited sludge has a water content of about 90%, and is removed at the center of the basins by means of circular scrapers and effluent conduits. Desilting removes an estimated five sixths of the silt load, and saves about $1,000,000 annually in canal maintenance costs (32). The use of desilting works of this type does not seem applicable to the prevention of reservoir silting itself, and appears practicable only when reservoir sedimentation is not sufficient for the purpose. It seems probable that "desilting" on a large scale may be secured more economically by vegetation screens previously mentioned.

Therefore it appears that the most certain means of maintaining reservoir capacity is to prevent sediment from forming, or, after formation, to prevent its deposition in the lake.

Henry M. Eakin and Carl B. Brown (1) state that:

> "Main reliance for material and permanent conservation of reservoir resources must rest upon control of sediment production at primary sources through more widespread and effective application of established methods of erosion control."

Summary

To a great extent, evaluation of the sediment problem on a particular watershed remains a qualitative one, when the accuracy of available methods of analysis is compared with that of the more rigidly established branches of hydrology. The general aspects of the principal phases of reservoir sedimentation have been reviewed, perhaps too briefly, in an attempt to summarize the present status of knowledge and investigation. Recent developments attempt, with some success, to combine the empirical and rational procedures; their present complexity warrants retaining those empirical procedures which have been used successfully in the past.

Additional data should indicate the possibility of developing accurate "regional indexes" that may be applied to proposed reservoirs. Certainly, it would be desirable that a fund of data be accumulated in this field as in others involving natural phenomena. Although present methods of collecting data give a reasonable accuracy considering the highly variable factors involved, it is doubtful that engineers will be satisfied until the most practical and accurate methods of measurement and analysis are evolved. The required research and the collection of data are necessarily expensive and slow, but the return that may be reasonably expected should justify the expense. It is doubtful that total research costs to date could exceed a small percentage of the value of irreplaceable reservoir storage capacity already destroyed.

Knowledge of the physical quantities involved must lead to decisions concerning whether to provide additional storage space in a reservoir to insure its returning the investment costs and charges during its lifetime, or to undertake such other remedies as may be possible. The former course, if physically possible at a given site, is strictly an economic problem and usually can be evaluated easily. The latter course usually entails considerable additional expenditures, but it is also a sociological problem which can scarcely be neglected by an enlightened and progressive engineering profession.

The literature on the subject is extensive, and the Bibliography in Appendix II barely indicates the work accomplished or in progress. Yet unified data are meager, or often unavailable when the practicing engineer is suddenly faced with a problem on reservoir sedimentation. Often, then, he must make use of such approximate methods of analysis as readily present themselves, or, if he has sufficient time and funds in a unique case, make an independent investigation.

Conclusion

The present methods of studying sediment transportation and reservoir sedimentation are laying the foundations and accumulating the basic data required for a better understanding of the problem. Continued study should accelerate progress. Available data and methods, and known remedies, must suffice for immediate problems. It has been the writer's intention to review the problems involved and some of the solutions for them.

Acknowledgments

The writer wishes to express his appreciation to all those listed in the Bibliography, and to those not listed, whose thoughts or ideas he may have assimilated and appropriated in a rather extensive reading of literature on the subject. He is further indebted to Lt.-Col. Harland C. Woods, M. Am. Soc. C. E., U. S. Engineer Office, Buffalo, N. Y., for his critical review of this paper, which was originally prepared early in 1942 for use within the U. S. Engineer Department. Revision for publication has involved practically complete rewriting, and some additional data and comments. The opinions expressed herein are the writer's and do not necessarily reflect the attitude of the U. S. Engineer Department.

APPENDIX I

Notation

The following letter symbols, used in the paper, conform essentially to American Standard Letter Symbols for Hydraulics,[3] prepared by a Committee of the American Standards Association, with Society representation, and approved by the Association in 1942:

[3] ASA—Z10.2-1942.

b = mean width of a stream; as a subscript it denotes "bed load";
C = general symbol for coefficients:
C_b = a constant in Eq. 5b, depending on the specific gravity and mechanical composition of the bed material;
C_k = an empirical coefficient in Eq. 1, depending on the kind of silt;
C_s = an empirical coefficient in a suspended sediment-rating curve;
C_T = "trap efficiency" coefficient for the reservoir;
c = velocity of a falling particle of sediment; as a subscript, c denotes "critical";
D = diameter: D_g = grain diameter;
d = a subscript denoting "total depth";
E = annual rate of erosion, or sediment production, on a watershed;
e = base of Napierian logarithms = 2.718;
G = rate of sediment movement, subscripts, b and s denoting "bed load" and "suspended load";
g = acceleration of gravity; as a subscript, g denotes sediment grain;
I = "regional index," an empirical coefficient in Eq. 6;
i = a subscript denoting "instantaneous";
N = sediment concentration, by weight, at any point in a vertical, at a distance y above the bottom: N_a = measured sediment concentration, by weight, at a sampling point in a vertical, distant y_a above the bottom;
n = Manning's roughness coefficient;
o = a subscript denoting the state at which bed-load movement begins;
P = ratio of mean sediment concentration in a vertical to sediment concentration at the bottom;
Q = total rate of water discharge: Q_i = total instantaneous discharge;
q = rate of water discharge per unit width of stream: q_o = value of q at which bed-load movement begins;
R = a subscript denoting "reservoir";
S = slope of the energy gradient of the stream at the time of measurement;
S_R = storage capacity of a reservoir: ΔS_R = annual rate of reservoir sedimentation;
s = a subscript denoting "suspended sediment";
T = a subscript indicative of the "sediment trap efficiency" of a reservoir;
t = dimensionless parameter in Eqs. 2 and 3;
V = velocity, subscripts m and c denoting "mean" and "critical," respectively;
W = total weight of sediment within a given size range in a unit width of stream;
x = a general symbol for exponent, defined in each case;
y = distance of any point in a vertical above the stream bed, definite points being further defined by subscripts;
z = relative depth of a point above the bottom $= \dfrac{y}{y_d}$;
Δ = annual rate; see ΔS_R.

APPENDIX II

Bibliography

(1) **"Silting of Reservoirs,"** by Henry M. Eakin (revised by Carl B. Brown), *Technical Bulletin No. 524*, U.S.D.A., 1939. (*a*) Fig. 19, facing p. 98. (*b*) Table 13, pp. 120–123.

(2) Discussion by Robert Horton of **"Dynamics of Water Erosion on Land-Surfaces,"** by Leonard Schiff and Robert E. Yodey, *Transactions*, Am. Geophysical Union, 1941, pp. 287–302.

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PAPERS

STRENGTH OF SLENDER BEAMS

BY GEORGE WINTER,[1] ESQ.

SYNOPSIS

Slender beams loaded in the usual manner tend to fail laterally at stresses often far below the yield point of the material. This fact is provided for in design specifications by reducing the basic allowable stress in the extreme compression fiber for beams in which the ratio of length to width is greater than a prescribed maximum. The specified reduction is based more or less on semi-empirical considerations rather than on strict analytical study.

In this paper the ultimate strength of slender beams is determined by rational methods similar to those generally accepted in column design, due consideration being given to highly probable imperfections in loading and beam shape. It is shown that these methods, whose value in column analysis is well established, can be applied with comparative ease to the fundamentally more complex beam problem.

Beam curves, similar in character to the well-known column curves, serve to illustrate the influence on the ultimate strength of such factors as slenderness ratio, shape of cross section, and degree of imperfection. In addition, a discussion is given of the influence of the character of the load, of intermediate bracing, and of elastic restraint at the end connections.

GENERAL

It has long been recognized that transversely loaded slender beams may fail by lateral buckling at stresses far below the yield point or the ultimate stress of the material (1).[2] In recent years considerable attention has been devoted to investigating the magnitude of those loads (critical loads) which cause beams of given shapes and dimensions to become unstable. These investigations apply only to very slender beams which buckle at stresses below the proportional limit. Therefore, their application to structural design is limited to very long

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[2] Numerals in parentheses, thus: (1), refer to the corresponding items in the Bibliography (Appendix I).

and narrow beams, just as the Eüler column formula gives reliable information on the carrying capacity only of very slender compression members. The designer, however, needs to know the allowable working stresses, with a given factor of safety, for beams of any dimensions. To meet this need, in the absence of more reliable information pertaining to beams of moderate slenderness, semi-empirical design specifications have been developed in various ways (2). Thus, the compression flange of a steel beam is frequently regarded as a strut free to buckle laterally, and the supposedly safe extreme fiber stress for this flange is obtained by applying a suitable column formula to the imaginary strut. It is evident that such a procedure represents, at best, a rather crude approximation, since the effect of the joint action of the compression flange with the remainder of the section, and particularly the influence of the torsional rigidity of the beam, are neglected entirely.

The situation is comparable to that which, years ago, prevailed in column design when the Eüler formula was accepted for columns of great slenderness and different kinds of semi-empirical transition curves were recommended for columns of moderate slenderness. In the field of compression members, considerable progress toward a more rational design has been made in recent years through acceptance of the secant formula or similar approaches (3)(4), as a criterion for the design of columns of any slenderness ratio. These methods have in common that they are based on a definite degree of inaccuracy of loading or of crookedness of the column. They allow a rigorous determination of the magnitude of that compression stress, $\frac{P}{A}$, which, for the given imperfections, will stress the outer fiber to the yield point or to any other given maximum stress. Column design curves and formulas are now generally based on this $\frac{P}{A}$-value.

This paper attempts to develop a similar rational basis for the design of slender beams. Paralleling the column methods, it assumes a given amount of inaccuracy of loading and proceeds to determine the magnitude of that applied stress, $\frac{M\,c}{I}$, which will stress to the yield point or any other given maximum value that point of the section farthest removed from the centroid of the section (for example, for I-beams the edges of the flanges). This applied stress can serve as a reliable design criterion just as the $\frac{P}{A}$-value obtained from the secant formula does in column design.

An analysis of doubly symmetrical solid and hollow sections, like rectangular or box-beams, is made first. This analysis is then extended to include the more involved symmetrical I-beam sections. From the resulting data, curves are drawn for various degrees of inaccuracy of loading and for different shapes of cross sections. These curves show the relation between the magnitude of that applied stress $s = \frac{M\,c}{I}$ which results in yield point stress s_y on the most remote points of the section and the slenderness ratio $\frac{L}{b}$ (L = span, b = width). By

applying a given factor of safety, these curves can be redrawn to give the allowable bending stress directly as a function of the slenderness ratio.

In addition to this part of the study, several other related factors are investigated. One of these is the influence of the character of the load. For beams stressed below the proportional limit, the critical moment M_c which causes the beam to buckle laterally depends not only on the shape and material of the beam but also on the type of loading (5). Although a complete treatment of this question is beyond the scope of this paper, a brief discussion of the magnitude of its effect is given.

The critical loads and ultimate stresses for slender beams are derived for freely supported ends, just as the Eüler and the secant formulas apply only to pin-ended columns. Most beams, however, are restrained elastically at their ends, in various degrees. The influence of such end restraints may be provided for in the same manner as for columns—namely, by introducing an equivalent reduced length instead of the actual span.

Notation.—The letter symbols in this paper are defined where they first appear (unless the definition is self-evident) and are assembled for convenience of reference in Appendix II.

Stresses in Slender Beams

In determining stresses in beams subject to transverse loading, generally it is assumed that the loading plane coincides with one of the principal planes of the beam. If the loading plane is inclined with respect to one of the principal axes, the moment is resolved into two components, M_x and M_y (see Fig. 1(*a*))

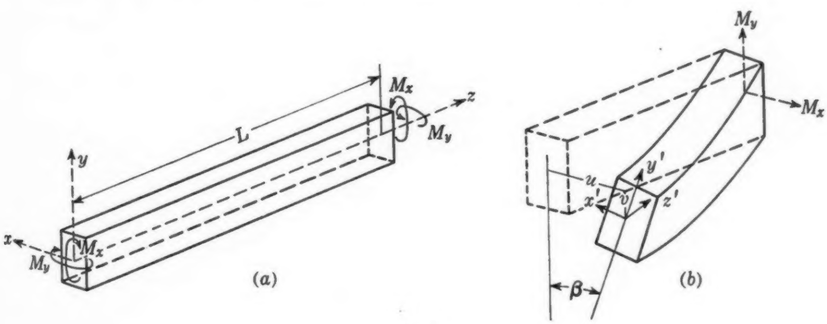


Fig. 1

and the maximum stress is computed from the equation

$$s_m = \frac{M_x\, c_{x'}}{I_{x'}} + \frac{M_y\, c_{y'}}{I_{y'}} \quad \text{(1)}$$

This formula is reasonably accurate for rather short beams with small ratios of $\frac{L}{b}$. However, for slender beams the actual maximum stress is larger than that given by Eq. 1, because a beam so loaded twists as it bends (see Fig. 1(*b*)). The principal axes x' and y' of the cross section anywhere along the span are no

longer parallel to the x-axis and y-axis, respectively, the inclination between these two sets of axes changing along the length of the beam. Consequently, the bending moments about the principal axes of the section are no longer M_x and M_y, but are the projections of these moments in the x' and y' directions. Therefore, they are dependent on the angle β. It will be shown that the increase of s_m due to the twist becomes very significant for slender beams; and, in fact, it may be regarded as the principal factor in relating allowable stresses to slenderness ratios $\frac{L}{b}$.

Inaccuracies of loading or shape are inevitable in beams just as they are in columns. In view of the influence of β on s_m it is proposed to assume that the plane of loading is slightly inclined with respect to the y-axis (Fig. 1(*a*)) and to investigate the effect of such inaccuracy of loading on beams of any slenderness in pure bending.

To determine the stresses in a section subtending the angle β with the y-axis it is necessary to project M_x and M_y into the x' and y' directions. With u and v representing the displacements of the centroid in the x-direction and y-direction, respectively, the direction cosines between the two sets of axes (5*a*) are as given in Table 1. In computing the values of this table advantage was taken of the fact that u, v, and β are small quantities. For this reason terms above the first order are negligible. In addition to the values for the x'-axis and y'-axis, cosines are also given with respect to the z'-axis (see Fig. 1(*b*)), since these will be used subsequently.

TABLE 1.—DIRECTION COSINES BETWEEN TWO SETS OF AXES

Axes	x	y
x'	1	β
y'	$-\beta$	1
z'	$\frac{du}{dz}$	$\frac{dv}{dz}$

With the vectors of M_x and M_y as shown in Fig. 1(*b*) the moments about the principal axes are

$$M_{x'} = - M_x + \beta M_y \qquad (2a)$$

and

$$M_{y'} = \beta M_x + M_y \qquad (2b)$$

The absolute magnitude of the maximum stress is obtained from the equation

$$s_m = \frac{M_{x'} c_{x'}}{I_{x'}} + \frac{M_{y'} c_{y'}}{I_{y'}} \qquad (3a)$$

in which $I_{x'}$, $I_{y'}$, $c_{x'}$, and $c_{y'}$ are, respectively, the moments of inertia about, and the distances to the outer fiber from, the x' and y' axes of the section. Substitution of Eqs. 2 in Eq. 3*a* gives

$$s_m = \frac{c_{x'}}{I_{x'}} (M_x - \beta M_y) + \frac{c_{y'}}{I_{y'}} (\beta M_x + M_y) \qquad (3b)$$

For deep beams ($I_{x'} \gg I_{y'}$) with but slightly inclined loadings ($M_x \gg M_y$), the term $- \frac{\beta M_y c_{x'}}{I_{x'}}$ is of higher order than the remainder of the terms. Consequently, for such conditions (of primary interest) this term can be neglected.

Then

$$s_m = \frac{M_x\, c_{x'}}{I_{x'}} + \frac{M_y\, c_{y'}}{I_{y'}} + \beta\, \frac{M_x\, c_{y'}}{I_{y'}} \qquad (3c)$$

A comparison of Eq. 3c with the usual approximate Eq. 1 shows that the two differ by the term $\frac{\beta\, M_x\, c_{y'}}{I_{y'}}$ which is due to the twist β. To determine s_m it is necessary first to compute β.

The Secant Formula for Rectangular and Box-Beams

For small angles β, neglecting terms of higher order, the curvature $\frac{d^2u}{(dz')^2} = \frac{d^2u}{dz^2}$ and the curvature $\frac{d^2v}{(dz')^2} = \frac{d^2v}{dz^2}$. Consequently, resolution of the applied end moments M_x and M_y in the x', y', and z' directions yields the three simultaneous equations of equilibrium (6):

$$M_{y'} = \beta\, M_x + M_y = E\, I_{y'}\, \frac{d^2u}{dz^2} \qquad (4a)$$

$$M_{x'} = -\, M_x + \beta\, M_y = -\, E\, I_{x'}\, \frac{d^2v}{dz^2} \qquad (4b)$$

and

$$M_{z'} = -\, \frac{du}{dz}\, M_x + \frac{dv}{dz}\, M_y = G\, K\, \frac{d\beta}{dz} \qquad (4c)$$

in which K is the torsional constant of the section (7). To eliminate u and v, Eq. 4c is differentiated and $\frac{d^2u}{dz^2}$ and $\frac{d^2v}{dz^2}$ substituted from Eqs. 4a and 4b. Thus,

$$\frac{d^2\beta}{dz^2} + \left(\frac{M^2{}_x}{R_{y'}} + \frac{M^2{}_y}{R_{x'}}\right)\beta = M_x\, M_y \left(\frac{1}{R_{x'}} - \frac{1}{R_{y'}}\right) \qquad (5)$$

in which $R_{x'} = E\, G\, K\, I_{x'}$; and $R_{y'} = E\, G\, K\, I_{y'}$, E being Young's modulus and G the shearing modulus. With

$$\alpha^2 = \frac{M^2{}_x}{R_{y'}} + \frac{M^2{}_y}{R_{x'}} \qquad (6a)$$

and

$$\gamma = M_x\, M_y \left(\frac{1}{R_{x'}} - \frac{1}{R_{y'}}\right) \qquad (6b)$$

the general solution of Eq. 5 is

$$\beta = C_1 \cos \alpha\, z + C_2 \sin \alpha\, z + \frac{\gamma}{\alpha^2} \qquad (7)$$

in which the constants C_1 and C_2 are determined from the end conditions. If the beam is "freely supported" (that is, if the ends are free to rotate about the x-axis and y-axis, while rotation about the z-axis is prevented), the end conditions are: $\beta_{z=0} = \beta_{z=L} = 0$; or—

$$\beta = \frac{\gamma}{\alpha^2}\left[1 - \left(\cos \alpha\, z + \sin \alpha\, z \tan \frac{\alpha\, L}{2}\right)\right] \qquad (8a)$$

From the coefficient

$$\frac{\gamma}{\alpha^2} = \frac{M_y}{M_x} \frac{\frac{I_{y'}}{I_{x'}} - 1}{\left[1 + \left(\frac{M_y}{M_x}\right)^2 \frac{I_{y'}}{I_{x'}}\right]} \qquad (8b)$$

it is seen that any symmetrical beam, subject to unsymmetrical bending, twists as it bends. In fact, $\beta = 0$ only if $I_{x'} = I_{y'}$. The physical significance of this result is this: It is well known that the direction of the deflections of beams subject to unsymmetrical bending is different from that of the applied bending moment. Consequently, the normal to the plane of any cross section is not perpendicular to the vector of the applied moment. The projection of this vector in the direction of the section-normal is equal to the twisting moment at that particular section. It vanishes only if $I_{x'} = I_{y'}$, as, for example, for quadratic or circular sections, since beams of such shape deflect in the plane of the applied moment.

In practical applications the influence of β becomes important only for deep beams $\left(\text{that is, when } \frac{I_{y'}}{I_{x'}} \ll 1\right)$. Eqs. 6 may be rewritten in the form

$$\alpha^2 = \frac{M^2{}_x}{R_{y'}} \left[1 + \left(\frac{M_y}{M_x}\right)^2 \frac{I_{y'}}{I_{x'}}\right] \qquad (9a)$$

and

$$\gamma = \frac{M_x\, M_y}{R_{y'}} \left[\frac{I_{y'}}{I_{x'}} - 1\right] \qquad (9b)$$

If terms of the order $\frac{I_{y'}}{I_{x'}}$ as compared with 1 are neglected,

$$\beta = \frac{\gamma_1}{\alpha^2{}_1} \left[1 - \left(\cos \alpha_1 z + \sin \alpha_1 z \tan \frac{\alpha_1 L}{2}\right)\right] \qquad (10)$$

in which,

$$\alpha^2{}_1 = \frac{M^2{}_x}{R_{y'}} \qquad (11a)$$

and

$$\gamma_1 = -\frac{M_x\, M_y}{R_{y'}} \qquad (11b)$$

Eqs. 10 and 11 also can be obtained directly if, in Eq. 4*c*, the influence of v on β is omitted (it actually is negligible in deep beams). Then, instead of the three Eqs. 4 one has simply Eq. 4*a* and the modified expression (see Eq. 4*c*),

$$M_{z'} = -\frac{du}{dz} M_x = G\, K \frac{d\beta}{dz} \qquad (12)$$

Eq. 4*b* represents the curvature in the direction of the main bending moment and is independent of Eqs. 4*a* and 12. The simultaneous solution of Eqs. 4*a* and 12 is identical with Eq. 10. From the comparison of the approximate

coefficient

$$\frac{\gamma_1}{\alpha^2_1} = -\frac{M_y}{M_x} \qquad (13)$$

for deep beams with the exact expression Eq. 8*b*, it is seen that in fact only terms of order $\frac{I_{y'}}{I_{x'}}$ have been neglected.

The maximum value of β occurs at $z = \frac{L}{2}$ and it is consequently at this place that the third term in Eq. 3*c* becomes most important. From Eq. 10, with $z = \frac{L}{2}$,

$$\beta_m = \frac{\gamma_1}{\alpha^2_1}\left(1 - \sec\frac{\alpha_1 L}{2}\right) \qquad (14a)$$

or, with Eq. 13,

$$\beta_m = -\frac{M_y}{M_x}\left(1 - \sec\frac{\alpha_1 L}{2}\right) \qquad (14b)$$

Substitution of this value of β in Eq. 3*c* yields

$$s_m = \frac{M_x c_{x'}}{I_{x'}}\left(1 + \frac{M_y c_{y'} I_{x'}}{M_x c_{x'} I_{y'}} \sec\frac{\alpha_1 L}{2}\right) \qquad (15)$$

On the other hand

$$\bar{s} = \frac{M_x c_{x'}}{I_{x'}} \qquad (16)$$

is the direct bending stress for the ideally straight beam loaded exactly in the y-direction; that is, for vertically loaded beams it is the only stress usually considered by the designer. Also, with $(s)_y = \frac{M_y c_{y'}}{I_{y'}}$—

$$k = \frac{M_y c_{y'}}{I_{y'}} \frac{I_{x'}}{M_x c_{x'}} = \frac{(s)_y}{\bar{s}} \qquad (17)$$

is a convenient measure of the inaccuracy of loading, comparable to $\frac{e\,c}{r^2}$ in column design (3)(4). Finally, from Eq. 11*a*,

$$\frac{\alpha_1 L}{2} = \frac{L}{2}\frac{M_x}{\sqrt{R_{y'}}} = \frac{L\,\bar{s}\,I_{x'}}{h\,\sqrt{E\,G\,I_{y'}\,K}} = \frac{L}{b}\frac{\bar{s}}{\sqrt{E\,G}}A' \qquad (18)$$

in which A' is a non-dimensional characteristic of the cross section

$$A' = \frac{I_{x'}}{\sqrt{I_{y'}\,K}}\frac{b}{h} \qquad (19)$$

With these simplifications Eq. 15 may now be rewritten as follows:

$$s_m = \bar{s}\left[1 + k \sec\left(\frac{L}{b}\frac{\bar{s}}{\sqrt{E\,G}}A'\right)\right] \qquad (20)$$

For a deep rectangular section with $I_{x'} = \frac{b\,h^3}{12}$, $I_{y'} = \frac{b^3\,h}{12}$, $K = \frac{b^3\,h}{3}$, A' has the particularly simple value

$$A' = \frac{h}{2\,b} \qquad (21)$$

For other types such as box-beams, expressions for A' are obtained easily from Eq. 19.

The practical problem, then, is to use Eq. 20 for finding pairs of values $\bar{s}$ and $\frac{L}{b}$ such that the maximum stress s_m has a prescribed value. In particular, the ultimate strength of a steel beam will be reached when s_m is equal to the yield point s_y. (Actually the carrying capacity of a beam is not yet reached when the diagonally opposite edges are stressed to the yield point. The main part of the section is then still stressed to a smaller amount and the beam will sustain a further load increase (8). For this same reason the secant formula for columns gives values somewhat below the ultimate capacity of the strut, especially for small $\frac{L}{r}$-values (4). It is customary, however, to disregard this strength reserve and to consider as the limiting load that load at which yield-point stresses occur in any portion of the section. This is particularly justified for dynamic loading.) To make $s_m = s_y$, Eq. 20 is conveniently written as

$$\bar{s} = \frac{s_y}{1 + k \sec\left(\frac{L}{b}\frac{\bar{s}}{\sqrt{E\,G}}\,A'\right)} \qquad (22)$$

which, in form, is analogous to the secant formula for columns. Thus, using Eq. 22, it is possible to plot $\bar{s}$ versus $\frac{L}{b}$ for any given yield point s_y and any degree of inaccuracy of loading k. It is seen that in addition to s_y and k, the $\left(\bar{s} - \frac{L}{b}\right)$-relation also depends on the characteristic A' of the cross section. For rectangular beams, with $A' = \frac{h}{2\,b}$, the following particularly simple formula is obtained:

$$\bar{s} = \frac{s_y}{1 + k\left(\frac{L\,h}{2\,b^2}\frac{\bar{s}}{\sqrt{E\,G}}\right)} \qquad (23)$$

Just as in the case of columns, any desired factor of safety is easily applied to these secant formulas, and results in curves giving the allowable working stress as a function of $\frac{L}{b}$ for given k and A'.

The Secant Formula for I-Beams

The formulas discussed in the preceding section apply only to doubly symmetrical solid or hollow cross sections. In deriving similar equations for sym-

metrical **I**-beams account must be taken of the additional torsional resistance of such beams due to lateral bending of the flanges. Instead of Eqs. 4, one has then (6):

$$M_{y'} = \beta\, M_x + M_y = E\, I_{y'} \frac{d^2u}{dz^2} \qquad (24a)$$

$$M_{x'} = -\, M_x + \beta\, M_y = -\, E\, I_{x'} \frac{d^2v}{dz^2} \qquad (24b)$$

and

$$M_{z'} = -\frac{du}{dz} M_x + \frac{dv}{dz} M_y = G\, K \frac{d\beta}{dz} - E\, I_{y'} \frac{h^2}{4} \frac{d^3\beta}{dz^3} \qquad (24c)$$

in which the second term on the right side of Eq. 24*c* represents the contribution of the flanges to the torsional rigidity.

The end conditions for the freely supported beam are

$$\beta_{z=0} = \beta_{z=L} = \left(\frac{d^2\beta}{dz^2}\right)_{z=0} = \left(\frac{d^2\beta}{dz^2}\right)_{z=L} = 0 \qquad (25)$$

The last two conditions express the fact that the longitudinal stresses due to warping of the beam vanish at the free ends. The solution satisfying Eqs. 24 and 25, as derived jointly by D. F. Gunder and the author, is

$$\beta = q \left[p_2 \left(-\sin m_1 z \tan \frac{m_1 L}{2} - \cos m_1 z \right) + p_1 \left(\sinh m_2 z \tanh \frac{m_2 L}{2} - \cosh m_2 z \right) + 1 \right] \qquad (26)$$

in which

$$q = -\frac{\dfrac{M_x\, M_y}{E}\left(\dfrac{1}{I_{y'}} - \dfrac{1}{I_{x'}}\right)}{\dfrac{M^2{}_x}{E\, I_{y'}} + \dfrac{M^2{}_y}{E\, I_{x'}}} \qquad (27a)$$

$$m^2{}_1 = -\frac{2\, G\, K}{E\, I_{y'}\, h^2} + \sqrt{\frac{4\, G^2\, K^2}{E^2\, (I_{y'})^2\, h^4} + \frac{4}{h^2}\left[\frac{M^2{}_x}{E^2\, (I_{y'})^2} + \frac{M^2{}_y}{E^2\, I_{y'}\, I_{x'}}\right]} \qquad (27b)$$

$$m^2{}_2 = \frac{2\, G\, K}{E\, I_{y'}\, h^2} + \sqrt{\frac{4\, G^2\, K^2}{E^2\, (I_{y'})^2\, h^4} + \frac{4}{h^2}\left[\frac{M^2{}_x}{E^2\, (I_{y'})^2} + \frac{M^2{}_y}{E^2\, I_{y'}\, I_{x'}}\right]} \qquad (27c)$$

and

$$p_1 = \frac{m^2{}_1}{m^2{}_1 + m^2{}_2}\,; \qquad p_2 = \frac{m^2{}_2}{m^2{}_1 + m^2{}_2} \qquad (27d)$$

Since, as before, $M_x \gg M_y$ and $I_{x'} \gg I_{y'}$, the expression for q simplifies to

$$q = -\frac{M_y}{M_x} \qquad (28)$$

For the same reason, in Eqs. 27*b* and 27*c*, $\dfrac{M^2{}_y}{E^2\, I_{y'}\, I_{x'}}$ is negligible when compared

with $\frac{M^2_x}{E^2\,(I_{y'})^2}$. Multiplying and dividing $\frac{4}{h^2}\frac{M^2_x}{E^2\,(I_{y'})^2}$ in Eqs. 27*b* and 27*c* by $h^2\,(I_{x'})^2$ and factoring,

$$m^2_1 = \frac{2\,G\,K}{E\,I_{y'}\,h^2}\left[\sqrt{1 + \left(2\,\frac{\bar{s}}{G}\,\frac{I_{x'}}{K}\right)^2} - 1\right] \quad \text{(29a)}$$

and

$$m^2_2 = \frac{2\,G\,K}{E\,I_{y'}\,h^2}\left[\sqrt{1 + \left(2\,\frac{\bar{s}}{G}\,\frac{I_{x'}}{K}\right)^2} + 1\right] \quad \text{(29b)}$$

in which, as before (see Eq. 16), $\bar{s} = \frac{M_x\,h}{2\,I_{x'}}$ is the applied direct bending stress.

The maximum stress s_m is again computed from Eq. 3*c* by introducing the maximum value for β. From Eq. 26, for $z = \frac{L}{2}$, and with q from Eq. 28,

$$\beta_m = -\frac{M_y}{M_x}\left[1 - p_2 \sec\frac{m_1\,L}{2} - p_1 \operatorname{sech}\frac{m_2\,L}{2}\right] \quad \text{(30)}$$

Substitution of this expression in Eq. 3*c* gives for the maximum stress

$$s_m = \frac{M_x\,c_{x'}}{I_{x'}}\left[1 + \frac{M_y\,c_{y'}\,I_{x'}}{M_x\,c_{x'}\,I_{y'}}\left(p_2 \sec\frac{m_1\,L}{2} + p_1 \operatorname{sech}\frac{m_2\,L}{2}\right)\right] \quad \text{(31)}$$

or, with $\bar{s}$ and k from Eqs. 16 and 17

$$s_m = \bar{s}\left[1 + k\left(p_2 \sec\frac{m_1\,L}{2} + p_1 \operatorname{sech}\frac{m_2\,L}{2}\right)\right] \quad \text{(32)}$$

Convenient forms of the arguments are obtained by multiplying and dividing by b^2 the factor in front of the brackets of Eqs. 29, and by making use of the relation $G = \frac{E}{2\,(1 + \nu)}$. Then

$$\frac{m_1\,L}{2} = \frac{L}{2\,b}\sqrt{\frac{1}{1+\nu}\,B'\left\{\sqrt{1 + \left[\frac{4\,(1+\nu)\,\bar{s}}{E}\,C'\right]^2} - 1\right\}} \quad \text{(33a)}$$

and

$$\frac{m_2\,L}{2} = \frac{L}{2\,b}\sqrt{\frac{1}{1+\nu}\,B'\left\{\sqrt{1 + \left[\frac{4\,(1+\nu)\,\bar{s}}{E}\,C'\right]^2} + 1\right\}} \quad \text{(33b)}$$

in which ν is Poisson's ratio and B' and C' are two non-dimensional characteristics of the cross section:

$$B' = \frac{K\,b^2}{I_{y'}\,h^2} \quad \text{(34a)}$$

and

$$C' = \frac{I_{x'}}{K} \quad \text{(34b)}$$

Also, using Eqs. 33 in Eq. 27*d*, the following simple expressions are obtained

for p_1 and p_2:

$$p_1 = \frac{\sqrt{1 + \left[\frac{4\,(1+\nu)\,\breve{s}}{E}\,C'\right]^2} - 1}{2\sqrt{1 + \left[\frac{4\,(1+\nu)\,\breve{s}}{E}\,C'\right]^2}} \qquad (35a)$$

and

$$p_2 = \frac{\sqrt{1 + \left[\frac{4\,(1+\nu)\,\breve{s}}{E}\,C'\right]^2} + 1}{2\sqrt{1 + \left[\frac{4\,(1+\nu)\,\breve{s}}{E}\,C'\right]^2}} = 1 - p_1 \qquad (35b)$$

If, for abbreviation, Eqs. 33 are rewritten in the form

$$\frac{m_1\,L}{2} = \frac{L}{2\,b}\,D'_1; \qquad \text{and} \qquad \frac{m_2\,L}{2} = \frac{L}{2\,b}\,D'_2 \qquad (36)$$

then from Eq. 32, by making $s_m = s_y$, the secant formula for I-beams is obtained:

$$\breve{s} = \frac{s_y}{1 + k\left[p_2 \sec\left(\frac{L}{2\,b}\,D'_1\right) + p_1 \operatorname{sech}\left(\frac{L}{2\,b}\,D'_2\right)\right]} \qquad (37)$$

It is seen from Eq. 37 that, as in the case of rectangular or box-beams, the direct bending stress $\breve{s}$ which produces yield-point stress in the most remote fiber depends not only on k and the slenderness ratio $\frac{L}{b}$, but also on additional characteristics of the cross section, B' and C'. In fact (compare Eqs. 33 and 35), p_1, p_2, D'_1, and D'_2 all depend upon these characteristics.

Eq. 37 is not very convenient to evaluate and can be solved numerically by a cut-and-try process only. However, considerable simplification can be made in most cases. From Eqs. 33 it is seen that $\frac{m_1\,L}{2} < \frac{m_2\,L}{2}$. Consequently $\sec\frac{m_1\,L}{2} = \sec\left(\frac{L}{2\,b}\,D'_1\right) > \operatorname{sech}\left(\frac{L}{2\,b}\,D'_2\right) = \operatorname{sech}\frac{m_2\,L}{2}$. In addition, from Eqs. 35 it is seen that $p_1 < p_2$. Consequently,

$$p_2 \sec\left(\frac{L}{2\,b}\,D'_1\right) \gg p_1 \operatorname{sech}\left(\frac{L}{2\,b}\,D'_2\right) \qquad (38)$$

and therefore, in most cases, the last term in the denominator of Eq. 37 is negligible. The simplified form of the secant formula for I-beams is then

$$\breve{s} = \frac{s_y}{1 + k\,p_2 \sec\left(\frac{L}{2\,b}\,D'_1\right)} \qquad (39)$$

For beams of medium and large slenderness the error so introduced is entirely negligible. Only for beams of rather small slenderness does the term

$p_1 \operatorname{sech}\left(\frac{L}{2b} D'_2\right)$ become significant. For such short beams $\bar{s}$ approaches $\frac{s_y}{1+k}$. Numerical computations for various rolled steel **I**-sections showed that for $\frac{L}{b}$ corresponding to about $\bar{s} = 0.9 \frac{s_y}{1+k}$ the error caused by omitting the hyperbolic term amounts to from 2% to 5%, and increases for still larger values of $\bar{s}$ $\left(\text{smaller values of } \frac{L}{b}\right)$. Since, for such beams, the hyperbolic term is no longer negligible, as a first approximation $\frac{L}{b}$ is computed from Eq. 39. The value so obtained is somewhat too large. Therefore, a smaller value is

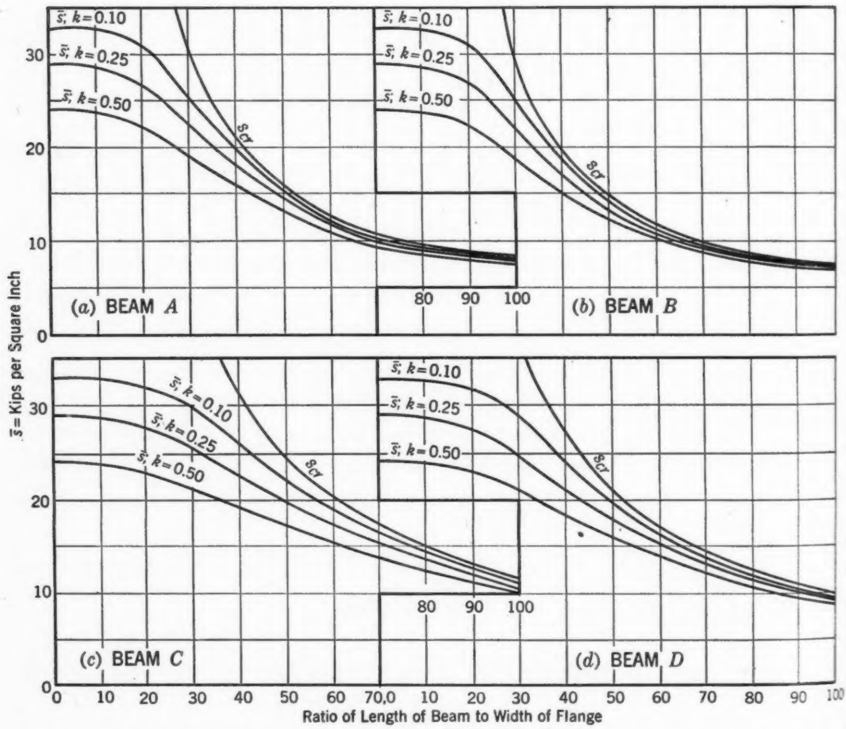


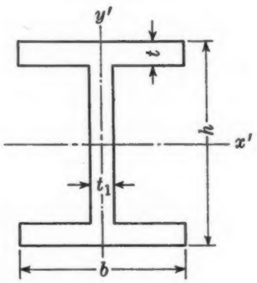
FIG. 2.—ULTIMATE STRESS $\bar{s}$ FOR A YIELD POINT OF 36,000 LB PER SQ IN. AND FOR VARIOUS DEGREES OF IMPERFECTION k; AND THE THEORETICAL BUCKLING STRESS s_c FOR AN IDEALLY PERFECT BEAM (SEE FIG. 3 FOR DIMENSIONS OF BEAMS)

substituted in the accurate Eq. 37 and the process is repeated until satisfactory accuracy is obtained. This method of successive approximation requires not more than two or three trials to give $\frac{L}{b}$ correct within a fraction of 1%.

Beam Curves

For columns, the influence of eccentric loading on the strength is demonstrated most easily by plotting $\bar{s}$ versus $\frac{L}{r}$ for given magnitudes of eccentricity (3)(4)(5). Design curves or formulas are then established by selecting a definite eccentricity most likely to fit practical conditions and applying a given factor of safety to the curve corresponding to that eccentricity (3)(9).

To allow for a similar approach in regard to beams, curves of the same nature for structural **I**-beams are drawn in Fig. 2. These curves were computed by means of Eqs. 37 and 39 for a yield-point stress s_y = 36,000 lb per sq in. Since, in the case of beams, $\bar{s}$ depends not only on the slenderness ratio $\frac{L}{b}$ but also on the characteristics B' and C' of the cross section (see Eqs. 34), it is not possible to draw a single set of curves for **I**-beams of any shape. For this reason, curves are given for four different **I**-beams, the dimensions of which are indicated in Fig. 3. These dimensions were chosen to cover, as far as possible, most of the range of standard rolled sections. Thus, the dimensions of beam A are approximately those of large heavy beams like 24-in., 100-lb **I**-beam or 18-in., 94-lb **I**-beam; those of beam B correspond to the lighter sections of the same depth such as 24-in., 79.9-lb **I**-beam or 18-in., 54.7-lb **I**-beam. The dimensions of beam C are approximately those of small, heavy beams like 15-in., 75-lb **I**-beam or 8-in., 25.5-lb **I**-beam, whereas those of beam D correspond to the lighter sections of the same depth, such as 15-in., 60.8-lb **I**-beam or 8-in., 18.4-lb **I**-beam. In Fig. 2 curves are drawn for three values of the degree of imperfection—k = 0.10, 0.25, and 0.50.



Beam	t_1	h	b
A	0.95 t	27.0 t	8.5 t
B	0.65 t	27.0 t	8.5 t
C	1.10 t	18.5 t	8.5 t
D	0.65 t	18.5 t	8.5 t

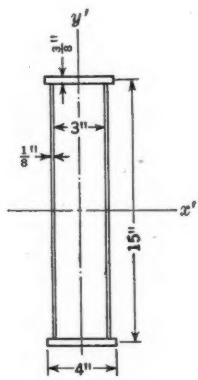


Fig. 3.—Dimensions of Beams Whose $\bar{s}$-Curves are Given in Figs. 2 and 4

In addition to the $\bar{s}$-curves the critical buckling stresses s_c are plotted for the four beams. The stress s_c is the maximum bending stress at which an ideally straight beam bent exactly in the plane of the web buckles laterally.

This s_c is easily computed from (10*a*):

$$M_c = E\frac{\pi^2}{L^2}\sqrt{I^2{}_o\,h^2 + 2\,C\,I_o\,\frac{L^2}{\pi^2}} \quad \ldots\ldots\ldots\ldots\ldots (40)$$

From this equation, by simple transformation, the critical stress is found to be

$$s_c = \frac{E\,\pi^2}{2\left(\frac{L}{b}\right)^2}\left(\frac{h}{b}\right)^2\sqrt{\left(\frac{I_{y'}}{2\,I_{x'}}\right)^2 + \frac{K\,I_{y'}}{2\,(1+\nu)\,I^2{}_{x'}}\left(\frac{L}{\pi\,h}\right)^2} \quad \ldots\ldots (41)$$

Eq. 41 was used for plotting the $\left(s_c - \frac{L}{b}\right)$-curves in Fig. 2. From these four sets of curves the following conclusions may be drawn:

Conclusion (*A*).—The general character of the $\bar{s}$-curves is the same for all beams. They start with $\bar{s} = \frac{s_y}{1+k}$ for $\frac{L}{b} = 0$ and approach the s_c-curves asymptotically at large values of $\frac{L}{b}$.

Conclusion (*B*).—For any given values of k and $\frac{L}{b}$ the corresponding value of $\bar{s}$ depends to a considerable degree upon the dimensions of the section. Of the four beams investigated beam B is the weakest laterally, showing the smallest $\bar{s}$ for a given k and $\frac{L}{b}$. It is followed in the order of increasing lateral strength by beams *A*, *D*, and *C*:

Beam	Values of $\bar{s}$ for $k = 0.25$ and $\frac{L}{b} = 50$
B	13,300
A	14,400
D	17,700
C	19,600

Conclusion (*C*).—The $\bar{s}$-curves approach the s_c-curves faster for the laterally weaker beams (AB), so that for such beams s_c is a fair measure of strength for large values of the slenderness ratio $\frac{L}{b}$. However, for laterally stronger beams (CD) the $\bar{s}$-curves fall considerably below the s_c-curves even for large values of $\frac{L}{b}$. Therefore, for such beams, the critical loads computed in the usual way (5)(10) for ideal straightness and ideal accuracy of loading are hardly to be regarded as a reliable criterion for the strength of structural beams with their usual inevitable imperfections.

Conclusion (*D*).—The strength of beams, particularly those of small and medium slenderness, depends mainly on the degree of imperfection k. In this connection it is interesting to note that the inclinations of the planes of loading corresponding to $k = 0.1$ to 0.5 are very small indeed. From Eq. 17 it is seen that

$$k = \frac{M_y}{M_x}\frac{S_{x'}}{S_{y'}} \quad \ldots\ldots\ldots\ldots\ldots\ldots (42)$$

in which $S_{x'}$ and $S_{y'}$ are the section moduli about the x' and y' axes, respectively. Consequently, the inclination ψ of the loading plane is

$$\psi = \frac{M_y}{M_x} = k \frac{S_{y'}}{S_{x'}} \qquad (43)$$

Since, for rolled **I**-sections, in general $S_{y'} : S_{x'} = 1 : 10$, the degrees of imperfection $k = 0.10$, 0.25, and 0.50 correspond respectively to inclinations of the loading plane of $\psi = 1 : 100$, $1 : 40$, and $1 : 20$. (This compares with a tolerance of 1 : 32 for "out of square or parallel" for American standard beams (11).)

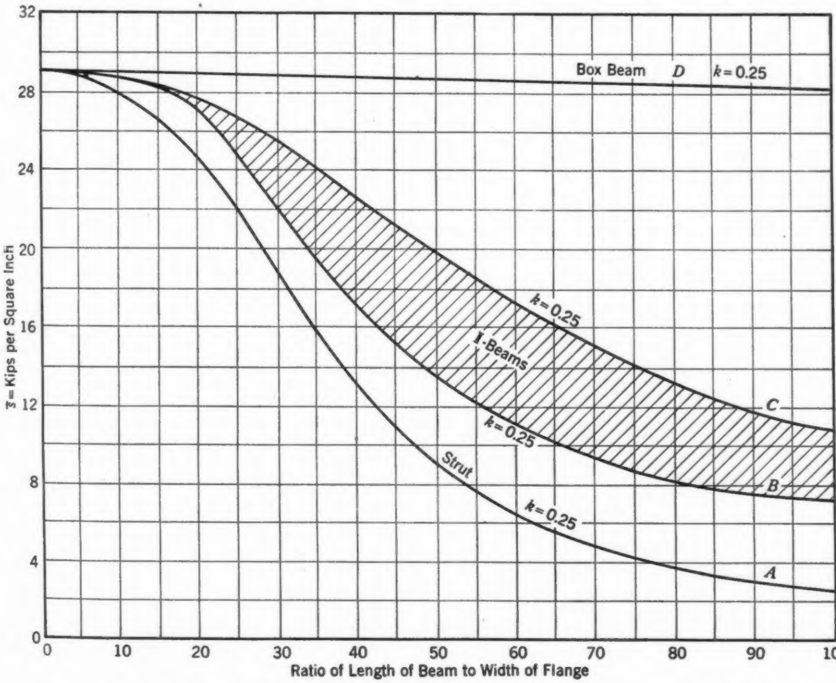


FIG. 4.—ULTIMATE STRESS $\bar{s}$ FOR A YIELD POINT OF 36,000 LB PER SQ IN. AND FOR $k = 0.25$
Curve A: Compression Flange Taken as Disconnected Compression Member Free to Buckle Laterally
Curve B: $\bar{s}$-Curve for Laterally Weak I-Beam B
Curve C: $\bar{s}$-Curve for Laterally Strong I-Beam C
Curve D: $\bar{s}$-Curve for Box-Beam (see Fig. 3 for dimensions of beams)

In Fig. 4, curves B and C for $k = 0.25$ have been drawn once more for the weakest and the strongest of the four beams. (The writer, emphatically, is not proposing to use $k = 0.25$ for design purposes. He selected this value for convenience exclusively, chiefly because design curves for columns are generally based on $\frac{e\,c}{r^2} = 0.25$. The proper degree of imperfection, k, to be used in design can be established only by extensive tests.) Additional computations

showed that the curves for most of the standard I-beams and for most of those wide-flange beams which are generally used as beams rather than columns fall between these two extreme curves. In Fig. 4, therefore, the shaded strip represents the region that is of the most practical interest to the designer. (For establishing design curves it is desirable to make a more complete survey of the entire range of rolled sections than the writer was able to undertake. Also, in addition to investigating only a limited number of beams, the writer simplified the shape of the cross sections as shown in Fig. 3(a), and also used an approximate formula—$K = \frac{1}{3} \Sigma\, w\, t^3$—for determining K. Therefore, the shaded zone in Fig. 4 represents the range of rolled sections in an approximate way only.)

Curve A in Fig. 4 represents the secant curve $\left(\frac{e\,c}{r^2} = 0.25\right)$ for the compression flange as a strut free to buckle laterally. This curve is seen to fall below any of the beam curves. Since considering the compression flange as a free strut means neglecting the additional resistance to twist and to lateral deflection of the remainder of the beam it is but natural that the values of s so obtained should be much smaller than those computed for the entire beam. It is common practice with many designers to limit the stress in the compression flange according to the column formula. Fig. 4 demonstrates clearly that such a procedure imposes unnecessarily severe restrictions on the allowable compression stress in bending.

Finally, in the same Fig. 4, curve D is given for a box-beam, the dimensions of which are shown in Fig. 3. (The dimensions of this beam are such as to represent a small-scale model of a heavy box girder of the type used in crane structures.) This curve was computed by means of Eq. 22. It illustrates the far greater lateral strength of slender box-beams as compared with I-beams. This difference is caused chiefly by the large resistance to twist characteristic of such sections, or, in other words, by the large value of K as compared with $I_{x'}$. Some design specifications are so worded that the same limitations on allowable compression stress in bending apply both to box-beams and to I-beams. The curves in Fig. 4 show the much greater rigidity of box-beams which is wholly ignored in specifications of this kind. In fact, it appears from these curves that if proper use is made of the large rigidity of box-beams, such sections are economically far more advantageous than I-sections for heavy structures of great span and considerable slenderness.

Beams Subject to Various Types of Transverse Loading

The foregoing conclusions were obtained by analyzing beams in pure bending; that is, subject to end moments only (see Fig. 1(a)). Structural beams, however, are acted upon by variously distributed transverse loads between supports rather than by end moments. It is to be asked, therefore, to what extent is an analysis of beams in pure bending applicable to transversely loaded structural members. An exact investigation of this question meets with practically insurmountable mathematical difficulties. It is easily possible, however, to explore the influence of the character of the load for the two extreme cases of very short beams on the one hand and very slender ones on

the other. From such an investigation conclusions can be drawn for beams of medium slenderness.

In Eq. 3*c*, the moments M_x and M_y may result from any kind of transverse loading or pure bending. For given values of M_x and M_y, however, the magnitude of β is different for different types of loading. Therefore, the only term in Eq. 3*c* that depends on the type of loading is the one involving β. For very short beams, however, the influence of β is insignificant since, in the limit, $\beta = 0$ for $\frac{L}{b} = 0$. For this limiting case, therefore, Eq. 3*c* transforms into Eq. 1 which is independent of the type of loading. Consequently, for $\frac{L}{b} = 0$,

$$\bar{s} = \frac{s_y}{1 + k} \qquad (44)$$

no matter how the beam is loaded. From Figs. 2 and 4 it is seen that all curves start at this value of $\bar{s}$ and all have horizontal tangents at that point. It can be stated, therefore, that the influence of the character of the load is nil for $\frac{L}{b} = 0$ and is negligibly small for beams of small slenderness ratio. If $\bar{M}_B$ represents the magnitude of M_x resulting in $\bar{s}_B$ for pure bending, and $\bar{M}_L$ the moment resulting in $\bar{s}_L$ for any particular type of transverse loading, then, for small values of $\frac{L}{b}$,

$$\bar{r} = \frac{\bar{s}_L}{\bar{s}_B} = \frac{\bar{M}_L}{\bar{M}_B} = 1 \qquad (45a)$$

For slender beams, on the other hand, the secant curves of Fig. 2 approach asymptotically the curves for the critical buckling stress. Therefore, for such beams, the ultimate applied stresses $\bar{s}$ as obtained from the secant formula depend on the type of loading in the same way as do the critical buckling stresses, s_c. Since the buckling loads of beams subject to various types of loading have been investigated extensively (5)(10)(12), it is possible by means of available formulas to establish the ratios

$$\bar{r}_c = \frac{s_{cL}}{s_{cB}} = \frac{M_{cL}}{M_{cB}} \qquad (45b)$$

in which M_{cB} is the critical moment for pure bending, M_{cL} that for any particular type of transverse loading, and s_{cB} and s_{cL} are the corresponding maximum fiber stresses. Then, because of the proximity of the $\bar{s}$-curves and the s_c-curves for slender beams, $\bar{s}_L \sim \bar{r}_c\, \bar{s}_B$. In this manner it is possible to investigate the influence of transverse loading for the two extreme cases of very short and of very slender beams.

To cover the most characteristic practical types of loading, the ratio $\bar{r}_c$ will be investigated for I-beams for a concentrated load at the center of the span, for uniformly distributed load, and for two equal loads applied at the quarter points.

The critical loads and moments of transversely loaded beams depend not only on the dimensions of the member and the type of loading, but also on the level, relative to the neutral axis, at which loads are applied (5)(10)(12). The ratios $\bar{r}_c$ for loads applied at the centroidal axis are independent of the configuration of the beam; for loads applied at other levels, however, $\bar{r}_c$ depends on the dimensions of the member. For this reason, loads applied at the centroid will be investigated first.

From Eq. 40, the critical moment for a symmetrical **I**-beam in pure bending is

$$M_{cB} = E \frac{\pi^2}{L^2} \sqrt{I^2{}_o\, h^2 + \frac{K}{1 + \nu}\, I_o\, \frac{L^2}{\pi^2}} \qquad (46)$$

in which I_0 is the moment of inertia of one flange about the axis of the web. For a concentrated load applied at the neutral axis (10*b*), the critical moment is

$$M_{cL} = \frac{P_c\, L}{4} = E\, \frac{3\, \pi^3}{L^2} \sqrt{\frac{1}{3\,(6 + \pi^2)}} \sqrt{I^2{}_o\, h^2 + \frac{K}{1 + \nu}\, I_o\, \frac{L^2}{h^2}} \qquad (47)$$

Consequently, for a center load applied at the neutral axis

$$\bar{r}_c = \frac{M_{cL}}{M_{cB}} = \frac{3\, \pi}{\sqrt{3\,(6 + \pi^2)}} = 1.36 \qquad (48a)$$

In the same manner it is found that (12*a*), with loads applied at the neutral axis, for uniformly distributed load,

$$\bar{r}_c = \frac{60\, \pi^2}{8\, \sqrt{30\,(\pi^4 + 45)}} = 1.13 \qquad (48b)$$

and, for quarter-point loads,

$$\bar{r}_c = \frac{17.3\, \sqrt{5.63}}{4\, \pi^2} = 1.04 \qquad (48c)$$

For center load applied at the top flange (10*c*),

$$M_{cL} = \frac{E\, I_o}{L} \frac{12\, \pi^2}{6 + \pi^2}$$

$$\left\{ - \frac{h}{L} + \sqrt{\frac{h^2}{L^2}\left[1 + \frac{\pi^2\,(6 + \pi^2)}{48}\right] + \frac{K}{(1 + \nu)\, I_o}\, \frac{6 + \pi^2}{48}} \right\} \qquad (49a)$$

From this equation it is seen that M_{cL} depends in this case on $\frac{h}{L}$ and $\frac{K}{I_o}$. To obtain an estimate of the average magnitude of $\bar{r}_c$ for rolled **I**-beams, the mean value of $\frac{K}{I_o}$ for the four beams A to D investigated previously will be used; that is $\frac{K}{I_o} = 0.20$. For reasons stated previously, $\bar{r}_c$ is of interest only in the region where the $\bar{s}$-curves are close to the s_c-curves. From Fig. 2, a reasonable value for this purpose is $\frac{L}{b} = 50$. Since, on the average, for rolled **I**-beams $\frac{h}{b} = 2.5$,

the corresponding value for $\frac{h}{L} = \frac{1}{20}$. For convenience of computation Eq. 46 is rewritten in the form

$$M_{cB} = \frac{E\,\pi^2\,I_o}{L}\sqrt{\frac{h^2}{L^2} + \frac{K}{(1+\nu)\,\pi^2\,I_o}} \qquad (49b)$$

Substitution of $\frac{K}{I_o} = 0.20$ and $\frac{h}{L} = \frac{1}{20}$ in Eqs. 49 gives, as an average value for rolled **I**-beams, for center load at the top flange, $\bar{r}_c = 1.10$. In a similar manner one obtains for center load at the bottom flange $\bar{r}_c = 1.66$.

By means of the methods just used for center load (12*a*), $\bar{r}_c$ for uniform load and for quarter-point load is easily established.

The values of $\bar{r}_c$ for the three types and the three levels of loading are given in Table 2.

TABLE 2.—VALUES OF $\bar{r}_c$ (EQ. 45*b*)

Load applied at:	Type of Loading		
	Uniform	Center	Quarter point
Top flange	0.96	1.10	0.90
Centroid	1.13	1.36	1.04
Bottom flange	1.34	1.66	1.21

For the slenderness ratio $\frac{L}{b} = 50$ for which Table 2 is computed, the stress $\bar{s}_L$ can now be determined from $\bar{s}$ by means of the expression $\bar{s}_L \sim \bar{r}_c\,\bar{s}_B$.

It is seen from Table 2 that if loads are applied at the top flange the value of $\bar{r}_c$ deviates from 1 by not more than 10%. On the other hand, for short beams $\bar{r} = 1$ (see Eq. 45*a*). For reasons of continuity it is reasonably safe to state, therefore, that $\frac{\bar{s}_L}{\bar{s}_B}$ is very close to 1 for beams of any slenderness and for any type of loading applied at the top flange. (The value $\frac{L}{b} = 50$ for which Table 2 was computed is arbitrary to some extent. However, to compute $\bar{r}_c$ for smaller values of $\frac{L}{b}$ seems inadvisable because of the increasingly large deviation of the $\bar{s}$-curves from the s_c-curves. If $\frac{L}{b} > 50$ had been chosen, the values for "load at centroid" would have stayed the same and the difference between them and those for "load at the top or bottom flange" would have been smaller. Therefore, for $\frac{L}{b} > 50$ the ratios r_c for loads applied at the top flange would still be close to 1 for beams within practically possible ratios of slenderness $\frac{L}{b}$.) Since $\frac{\bar{s}_L}{\bar{s}_B}$ is very close to 1, the influence of the character of the

loading on the magnitude of $\bar{s}$ is seen to be practically negligible if loads are applied at the top flange. The conclusion to be drawn from this discussion is that it appears reasonable to use the $\bar{s}$-curves for pure bending for all types of loading rather than to complicate design procedures by taking account of the secondary influence of the character of the load.

In the rather rare cases of loads being applied at the bottom flange such a procedure would be on the conservative side. This is illustrated clearly by Table 2 which indicates the greater stability of beams loaded at the bottom flange as compared with those loaded at the top flange.

The Torsional Constant K

From the preceding discussion the strength of slender beams is seen to depend on three properties of the cross section—the two principal moments of inertia $I_{x'}$ and $I_{y'}$ and the torsional constant K. Although the structural designer is fully familiar with moments of inertia, some remarks may be in order on the magnitude of K.

For determining the torsional constants of the four beams A to D investigated, the writer used the approximate formula

$$K = \tfrac{1}{3} \Sigma\, w\, t^3 \qquad (50)$$

in which w and t are, respectively, the width and the thickness of the three component rectangles of the sections. More detailed and accurate formulas for **I**-beams, taking account of the radii of fillets, the slope of the flanges, etc., as well as formulas for other types of cross sections have been developed and compiled by various authors (7)(13). These formulas hold only for monolithic members (rolled sections and the like).

The torsional constants K of built-up (riveted or welded) girders are smaller than those computed for the corresponding monolithic sections. This difference is due to relative slipping of the component parts of fabricated members. Since the strength of slender beams depends directly on the torsional rigidity, it is important to make proper allowance for the smaller value of K for built-up girders as compared with monolithic beams.

Elaborate experimental investigations of the magnitude of K for built-up members were conducted by J. Madsen (14) in 1941. Four **I**-girders were tested, two of them riveted and two welded, and four box-girders, of which three were riveted and one welded. The girders were of the type commonly used in crane construction. The results of these tests show that for riveted girders, both of box-section and **I**-section, the effective K amounts to from 30% to 41% of the torsional constant computed for the corresponding monolithic section. It appears, therefore, that for design purposes a value of K equal to one third of that of the monolithic section should be used for riveted girders. The welded **I**-beams gave effective K-values of 67% and 69% of the value computed for the solid section, whereas the percentage for the one welded box-beam tested by Mr. Madsen was 94%. For welded sections, consequently, a value of K equal to two thirds of that of the monolithic section appears to be safe for design computations.

The amount of experimental evidence on the torsional behavior of built-up members cannot yet be regarded as sufficient. A greater number of tests is desirable on solid web beams in order to provide collateral evidence to Mr. Madsen's results. In addition, to the writer's knowledge, the torsional properties of open web trusses have not yet been investigated experimentally. In view of the general tendency toward lighter and more slender steel and metal structures, the question of the torsional behavior of structural members is becoming increasingly important. Here a wide field for useful experimental work is open to the investigator.

The Influence of Lateral Restraints

The findings of the preceding parts of this paper apply to beams with freely supported ends; that is, to beams the ends of which are free to rotate about the x-axis and y-axis. However, beams are generally connected to other structural members in a way that provides some measure of elastic restraint at the supports. Also, long beams are frequently braced laterally at some intermediate points between supports which, with regard to lateral deflection, make them act as beams continuous over several supports, whereas, with respect to vertical deflections, they act as simply supported beams. Such measures increase the lateral stability of beams just as end restraints or intermediate bracing raise the ultimate load of a column above that given by the Eüler formula for hinged ends. It is the purpose of this last part of the paper to indicate the method of making allowance for the influence of such lateral restraints.

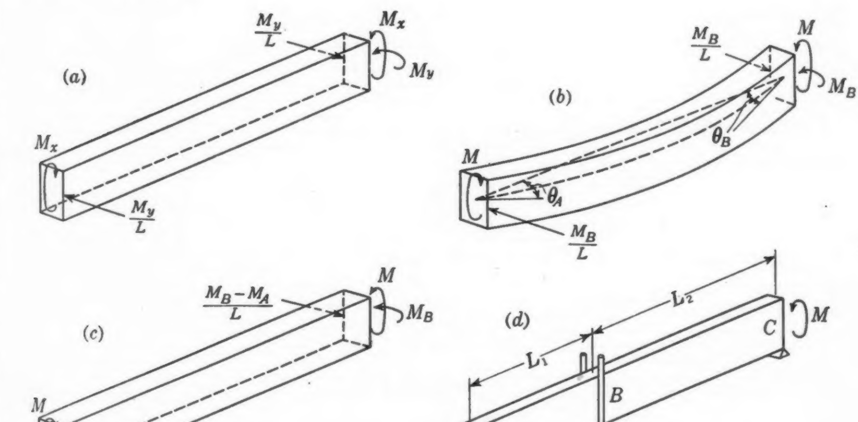


Fig. 5

This problem can be solved by first analyzing the case of a beam acted upon by end moments as shown in Fig. 5(a). With the same nomenclature as used heretofore, the differential equations of equilibrium, in this case,

become

$$M_{y'} = \beta\, M_x + M_y\, \frac{z}{L} = E\, I_{y'}\, \frac{d^2u}{dz^2} \quad \text{(51a)}$$

$$M_{x'} = -\, M_x + \beta\, M_y\, \frac{z}{L} = -\, E\, I_{x'}\, \frac{d^2v}{dz^2} \quad \text{(51b)}$$

$$M_{z'} = -\, \frac{du}{dz}\, M_x + \frac{dv}{dz}\, M_y\, \frac{z}{L} = G\, K\, \frac{d\beta}{dz} \quad \text{(51c)}$$

Elimination of u and v results in:

$$\frac{d^3\beta}{dz^3} + \frac{d\beta}{dz}\,\frac{1}{R_{z'}}\left(M^2{}_x + M^2{}_y\,\frac{z^2}{L^2}\,\frac{I_{y'}}{I_{x'}}\right) + \beta\,\frac{3\,M^2{}_y}{R_{y'}}\,\frac{z}{L^2}\,\frac{I_{y'}}{I_{x'}}$$
$$= \frac{M_x\, M_y}{L\, R_{y'}}\left(\frac{2\, I_{y'}}{I_{x'}} - 1\right) \quad \text{(52)}$$

Eq. 52 is not capable of solution in terms of simple functions. However, for the purpose of considering the stability of deep beams it is again permissible to neglect terms of order $\frac{I_{y'}}{I_{x'}}$ as compared with 1. For the same reason as in the preceding discussion (see comment on Eqs. 10 and 11), this is equivalent to neglecting the second term in Eq. 51*c*, which then becomes

$$-\, \frac{du}{dz}\, M_x = G\, K\, \frac{d\beta}{dz} \quad \text{(53)}$$

Eliminating u from Eqs. 51*a* and 51*c*:

$$\frac{d^2\beta}{dz^2} + \alpha^2{}_1\, \beta = \gamma_1\, \frac{z}{L} \quad \text{(54)}$$

in which α_1 and γ_1 are given by Eqs. 11. (If Eq. 54 is differentiated with respect to z—

$$\frac{d^3\beta}{dz^3} + \frac{d\beta}{dz}\,\frac{M^2{}_x}{R_{y'}} = -\, \frac{M_x\, M_y}{L\, R_{y'}} \quad \text{(55)}$$

A comparison of Eq. 55 with Eq. 52 shows that actually only terms of order $\frac{I_{y'}}{I_{x'}}$ have been neglected.)

If the beam is freely supported, the end conditions are

$$(\beta)_{z=0}\; (\beta)_{z=L} = 0 \quad \text{(56a)}$$

and the solution of Eq. 54, satisfying Eqs. 51*a*, 53, and 56*a*, is

$$\beta = \frac{M_y}{M_x}\left(\frac{\sin \alpha_1\, z}{\sin \alpha_1\, L} - \frac{z}{L}\right) \quad \text{(56b)}$$

For stability calculations it is convenient to use $\frac{du}{dz}$, the horizontal projection of the slope of the elastic curve. From Eqs. 53 and 56*b* this slope is

found to be

$$\frac{du}{dz} = -\frac{G\,K}{L}\frac{M_y}{M^2_x}\left(\frac{\alpha_1\,L\cos\alpha_1\,z}{\sin\alpha_1\,L} - 1\right) \qquad (57a)$$

In particular, at the ends

$$\left(\frac{du}{dz}\right)_{z=0} = -\frac{G\,K}{L}\frac{M_y}{M^2_x}\left(\frac{\alpha_1\,L}{\sin\alpha_1\,L} - 1\right) \qquad (57b)$$

and

$$\left(\frac{du}{dz}\right)_{z=L} = -\frac{G\,K}{L}\frac{M_y}{M^2_x}\left(\alpha_1\,L\cot\alpha_1\,L - 1\right) \qquad (57c)$$

As an example, consider the case of a beam in pure bending built in laterally at $z = L$ (rotation about an axis parallel to y prevented at $z = L$) and freely supported at $z = 0$. In this case

$$\left(\frac{du}{dz}\right)_{z=L} = 0; \qquad \text{and} \qquad \alpha_1\,L\cot\alpha_1\,L = 1 \qquad (57d)$$

This transcendental equation is satisfied by $\alpha_1\,L = 4.49\cdots$; and consequently, from Eq. 11*a*, the critical moment becomes

$$M_{xc} = \frac{4.49}{L}\sqrt{R_{y'}} \qquad (58)$$

It is worth noticing (5*b*) that the problem of a strut in compression fixed at one end and hinged at the other leads to the same transcendental expression as Eq. 57*d*.

Eqs. 57*b* and 57*c* make it possible to discuss the stability of elastically restrained beams in complete analogy with Professor Timoshenko's treatment of elastically restrained compression members (5).

If θ_A and θ_B represent the horizontal angles of rotation of the elastic line at the supports, positive as shown in Fig. 5(*b*), by suitable transformation Eqs. 57*b* and 57*c* may be rewritten as follows: Comparing Eq. 57*b*—

$$\theta_A = \frac{M_B\,L}{6\,E\,I_{y'}}\frac{3}{\frac{1}{2}\,\alpha_1\,L}\left(\frac{1}{\sin\alpha_1\,L} - \frac{1}{\alpha_1\,L}\right) \qquad (59a)$$

and, comparing Eq. 57*c*—

$$\theta_B = \frac{M_B\,L}{3\,E\,I_{y'}}\frac{3}{\alpha_1\,L}\left(\frac{1}{\alpha_1\,L} - \cot\alpha_1\,L\right) \qquad (59b)$$

This particular form is convenient because, from the ordinary beam theory, $\frac{M_B\,L}{6\,E\,I_{y'}}$ and $\frac{M_B\,L}{3\,E\,I_{y'}}$ are the end rotations of a beam subject to M_B only ($M_x = 0$). The terms in Eqs. 59*a* and 59*b* containing α_1, therefore, have the significance of magnification factors which represent the influence of M_x on the magnitude of the horizontal end rotation θ.

Professor Timoshenko has tabulated (5*e*) values of the functions:

$$\phi(a) = \frac{3}{a}\left(\frac{1}{\sin 2\,a} - \frac{1}{2\,a}\right) \qquad (60a)$$

and

$$\psi(a) = \frac{3}{2\,a}\left(\frac{1}{2\,a} - \cot 2\,a\right) \ldots \ldots \ldots \ldots (60b)$$

in which $2\,a = \alpha_1\,L$. Consequently

$$\theta_A = \frac{M_B\,L}{6\,E\,I_{y'}}\,\phi(a) \ldots \ldots \ldots \ldots (61a)$$

and

$$\theta_B = \frac{M_B\,L}{3\,E\,I_{y'}}\,\psi(a) \ldots \ldots \ldots \ldots (61b)$$

For a beam acted upon by moments at both ends as shown in Fig. 5(*c*), by superposition the angles of rotation are, consequently:

$$\theta_A = \frac{M_A\,L}{3\,E\,I_{y'}}\,\psi(a) + \frac{M_B\,L}{6\,E\,I_{y'}}\,\phi(a) \ldots \ldots \ldots \ldots (62a)$$

and

$$\theta_B = \frac{M_B\,L}{3\,E\,I_{y'}}\,\psi(a) + \frac{M_A\,L}{6\,E\,I_{y'}}\,\phi(a) \ldots \ldots \ldots \ldots (62b)$$

with

$$2\,a = \alpha_1\,L = \frac{M_x\,L}{\sqrt{E\,I_{y'}\,G\,K}} \ldots \ldots \ldots \ldots (63)$$

These expressions are exactly the same as the corresponding equations for compression members (5*c*) except for the different significance of *a*. In beams horizontally restrained at the ends, M_A and M_B represent the restraining moments. Consequently, by means of the functions $\phi(a)$ and $\psi(a)$ it is possible to determine the critical moments of elastically restrained beams by exactly the same methods used to calculate the critical thrust for elastically restrained compression members.

Example (*a*).—Suppose a beam in pure bending is braced horizontally at point B as schematically indicated by the guides in Fig. 5(*d*). There are no horizontal restraining moments at the ends, $M_A = M_C = 0$; and, for reasons of equilibrium, $M_{1B} = -\,M_{2B}$. Consequently:

$$\theta_{1B} = \frac{M_B\,L_1}{3\,E\,I_{1y'}}\,\psi(a) \ldots \ldots \ldots \ldots (64a)$$

and

$$\theta_{2B} = -\,\frac{M_B\,L_2}{3\,E\,I_{2y'}}\,\psi(a_2) \ldots \ldots \ldots \ldots (64b)$$

The continuity of the beam requires that $\theta_{1B} = \theta_{2B}$, which results in

$$\psi(a_1) + \frac{L_2\,I_{1y'}}{L_1\,I_{2y'}}\,\psi(a_2) = 0 \ldots \ldots \ldots \ldots (65)$$

If, for example, the beam is of constant cross section and $L_2 = 2\,L_1$, it follows that

$$\frac{\psi(a_1)}{\psi(2\,a_1)} = -\,2 \ldots \ldots \ldots \ldots (66)$$

From Professor Timoshenko's tables (5*e*) it is then found that $2\,a_1 = \alpha_1\,L = 1.93$. Consequently, from $\alpha_1 = \frac{M_x}{\sqrt{E\,I_{y'}\,G\,K}}$:

$$M_c = \frac{1.93\,\sqrt{E\,I_{y'}\,G\,K}}{L_1} = \frac{3.86\,\sqrt{E\,I_{y'}\,G\,K}}{L_2} \qquad (67)$$

The corresponding problem for a braced strut in compression is developed by Professor Timoshenko (5*d*), and the critical load for this case is

$$P_c = \frac{1.93^2\,E\,I}{L^2_1} = \frac{3.86^2\,E\,I}{L^2_2} \qquad (68)$$

Example (*b*).—As a second example consider the beam built in laterally at both ends. In this case $M_A = M_B$ and $\theta_A = \theta_B = 0$. From Eqs. 62, $\frac{\psi(a)}{3} + \frac{\psi(a)}{6} = 0$, and $\phi(a) = -\,2\,\psi(a)$; and from Professor Timoshenko's tables (5*e*) $2\,a = \alpha_1\,L = 2\,\pi$. Consequently,

$$M_c = \frac{2\,\pi\,\sqrt{E\,I_{y'}\,G\,K}}{L} \qquad (69)$$

Eq. 69 can also be obtained directly from Eqs. 10 and 12 by making $\frac{du}{dz} = 0$ at the ends. In general, then, the critical moment for a beam in pure bending can be expressed in the form

$$M_c = \frac{\pi\,\sqrt{E\,I_{y'}\,G\,K}}{L'} \qquad (70a)$$

just as the critical thrust for compression members is given by

$$P_c = \frac{\pi^2\,E\,I}{(L')^2} \qquad (70b)$$

In both cases L' is the "equivalent span" and is equal to the distance between the inflection points of the elastic curve. From the three examples computed herein (Eqs. 58, 67, and 69) it is apparent that the ratio $\frac{L}{L'}$ of the actual to the equivalent span for lateral buckling of beams is the same as the ratio $\frac{L}{L'}$ for struts in compression, with corresponding conditions of loading and restraint.

Consequently, in order to take account of any degree of lateral restraint in the design of slender beams one has only to reduce the actual span by an appropriate amount. The equivalent span L' so obtained is then used in the secant formulas or the corresponding curves instead of the actual span L.

The degree of end restraint provided by the usual types of riveted or welded end connections can hardly be determined analytically. For this reason, and also to establish the proper degree of imperfection k to be used in design, it is desirable to make systematic tests on beams under conditions simulating those in actual structures.

Summary

In this paper the writer endeavors to establish a rational basis for the design of slender beams. Just as in the case of column analysis, such design should be based on the occurrence of inevitable imperfections of shape and loading. The paper contains:

(*a*) The derivation of a secant formula for rectangular and box-beams and similar shapes;

(*b*) The derivation of a secant formula for I-beams;

(*c*) Illustrative beam curves which show the influence of variations of shape and degree of imperfection on the ultimate strength of slender beams (curves of this type should serve as design criteria, rather than the buckling load of the compression flange regarded as a free strut, as is frequently done);

(*d*) A discussion of the influence of the character of the load on the ultimate strength;

(*e*) A discussion of the magnitude of the torsional constant K and particularly its value for fabricated (riveted or welded) sections; and

(*f*) A method of accounting for the influence on the ultimate strength of additional restraints such as intermediate bracing, or complete or partial end restraint.

The writer does not propose definite design curves or formulas. To establish design specifications extensive experimental work appears to be necessary to determine the degree of imperfection and the amount of end restraint under service conditions.

Acknowledgment

For the valuable assistance of Messrs. D. F. Gunder and Robert Lewis, Jun. Am. Soc. C. E., respectively Associate Professor and Assistant Professor of Civil Engineering at Colorado State College, Fort Collins, Colo., and at the time, respectively Resident Doctor and McMullen Research Scholar at Cornell University, Ithaca, N. Y., the writer expresses his sincere appreciation. As stated, Professor Gunder collaborated with the writer in deriving Eq. 26; and Professor Lewis undertook the numerical computations for the curves in Figs. 2 and 4.

APPENDIX I

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APPENDIX II

Notation

The following letter symbols, adopted for use in the paper, conform essentially to American Standard Letter Symbols for Mechanics, Structural Engineering and Testing Materials,[3] prepared by a Committee of the American Standards Association, with Society representation, and approved by the Association in 1932. At the same time, an effort has been made to reconcile the notation of other authors in this field, particularly Professor Timoshenko.

A = cross-section area;

A', B', C', D' are non-dimensional characteristics of the cross section (see Eqs. 19, 34, 36);

$a = \frac{\alpha_1 L}{2}$ (see Eq. 63) for use in the Timoshenko tables (5e);

B' = (see A');

b = width of beam;

C = a constant (10) $= \frac{K}{2(1+\nu)}$; C_1 and C_2 are integration constants determined from the conditions at the ends of a beam;

C' = (see A');

c = distance from the neutral axis to the extreme fiber; $c_{x'}$ and $c_{y'}$ are the distances c measured perpendicular to the x' and y' axes, Fig. 1;

D' = (see A');

E = Young's modulus of elasticity;

e = eccentricity of a load;

G = shearing modulus of elasticity;

h = height of a beam cross section;

I = rectangular moment of inertia: $I_{x'}$ and $I_{y'}$ are the moments of inertia about the axes x' and y', Fig. 1; and I_o is the moment of inertia of one flange about the axis of the web;

K = a torsional constant;

k = degree of imperfection of loading and shape of a beam, defined in Eq. 17;

L = span length;

L' = equivalent span, or the distance between the inflection points of the elastic curve;

M = bending moment:

- M_c = critical moment;
- M_m = maximum moment;
- M_x, M_y, M_z, $M_{x'}$, $M_{y'}$, and $M_{z'}$ = the components of M on the axis denoted;
- M_B = bending moment due to pure bending;
- M_L = bending moment due to any kind of loading except pure bending;
- $\bar{M}$ = the magnitude of M that causes an ultimate applied unit stress $\bar{s}$;

m_1 and m_2 = constants first used in Eq. 26, defined in Eqs. 27 and 29;

[3] ASA—Z10a—1932.

P = a concentrated load: P_c = a critical concentrated load;
p_1 and p_2 = constants first used in Eq. 26, defined in Eqs. 27*d*;
q = a constant first used in Eq. 26, defined in Eq. 28;
R = $E\,G\,K\,I$, an abbreviation first used in Eq. 5;
r = least radius of gyration;
$\bar{r}$ = a ratio of ultimate stresses or moments: $\bar{r}_c$ = a ratio of critical stresses or moments;
s = stress per unit area:
s_c = critical unit stress;
s_m = maximum unit stress;
s_y = unit stress at the yield point;
s_B = unit stress due to pure bending;
s_L = unit stress due to any kind of loading except pure bending;
$(s)_y = \dfrac{M_y\, c_{y'}}{I_{y'}}$ = the applied bending stress due to M_y, for use in Eq. 17;
$\bar{s}$ = direct stress for ideally straight beam;
t = thickness; thickness of flange: t_1 = thickness of web;
u u and v (see Fig. 1) are horizontal and vertical displacement ordinates of a section of a beam after rotation through an angle β;
v = (see u);
w = width of component rectangles of an I-section;
x x, y, and z are distances measured parallel to the respective axes; x', y', and z' are the corresponding distances referred to the rotated section. As subscripts they refer a given symbol to the rotated section;
y = (see x);
z = (see x);
α = a constant first used in Eq. 7, defined in Eq. 6*a*: α_1 = an approximate value for α (Eq. 11*a*);
β = angle of twist;
γ = a constant first used in Eq. 7, defined in Eq. 6*b*: γ_1 = an approximate value for γ (Eq. 11*b*);
θ = horizontal angle of rotation of the elastic line at the supports, θ_A and θ_B referring to supports A and B, respectively;
ν = Poisson's ratio;
ϕ = "function of:" as in $\phi(a)$;
ψ = inclination of the loading plane; also, where indicated in the text, ψ = "function of:" as in $\psi(a)$.

Subscripts and Other Modifiers.—Used as subscripts, c = critical, m = maximum; A and B denote the ends of beam AB (except where B is specially defined as meaning "due to bending"); and L denotes "due to any other kind of loading except pure bending." Subscripts x, y, z, x', y', and z' refer to the corresponding axes. Primed symbols in general refer to a non-dimensional characteristic (see A') but they are also used to denote an equivalent value, as in L'. Numerical subscripts 1 and 2 serve to distinguish between two individual quantities of the same general nature, such as C_1 and C_2, m_1 and m_2, etc. Barred symbols in general refer to critical or ultimate values, except that $\bar{s}$ denotes a value for an ideally straight beam.

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AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

DISCUSSIONS

ENTRAINMENT OF AIR IN FLOWING WATER A SYMPOSIUM

Discussion

By Messrs. D. C. McConaughy, and T. J. Corwin, Jr.

D. C. McConaughy,[44] Esq.[44a]—Mr. Hall's paper treats a phase of hydraulics that is almost unexplored and, it is to be hoped, will stimulate research looking toward the solution of the problems involved.

The Bureau of Reclamation has been interested in this field in recent years in connection with spillways for high concrete dams and for earth dams, in which chutes designed for large volumes of water at high velocities form an important part.

In the absence of any theoretical development of a method of correlating and extending results of experiments on small structures, it is desirable that data be obtained on larger ones. Unfortunately, structures suitable for this purpose are rare. Conditions such as availability of water, a means of measuring the discharge, interference with power or irrigation requirements, etc., must be satisfied, which make it very difficult to conduct a satisfactory experimental program. The data used by the author on the Bureau of Reclamation tests on Kittitas chute were contained in informal memoranda which had not been released for publication because the accuracy of the velocity measurements was considered somewhat questionable and because it was felt that they would add little to existing knowledge on the subject. It was hoped that experiments over a wider range, which could be accepted without reservation, could be obtained before anything was published. However, in view of the publicity which has now been given to the experiments, a more complete description than Mr. Hall's should be given for the benefit of possible future users of the data.

Experiments for perfecting satisfactory apparatus and techniques for making the necessary velocity and depth measurements were started in the

Note.—This Symposium was published in September, 1942, *Proceedings*. Discussion on this Symposium has appeared in *Proceedings*, as follows: January, 1943, by Warren DeLapp, Jun. Am. Soc. C. E.; February, 1943, by Karl R. Kennison, M. Am. Soc. C. E.; March, 1943, by Messrs. Robert T. Knapp, and Carl E. Kindsvater; April, 1943, by Messrs. J. H. Douma, and Joe W. Johnson; and May, 1943, by J. C. Stevens, M. Am. Soc. C. E.

[44] Senior Engr., Bureau of Reclamation, U. S. Dept. of the Interior, Denver, Colo.

[44a] Received by the Secretary April 19, 1943.

hydraulic laboratory about 1937, first, on an existing model in which velocities up to 15 ft per sec could be obtained and, later, on another model specially built for the purpose, giving velocities up to 50 ft per sec. All known methods of measuring velocities were considered, and several were used experimentally. The salt-velocity method was finally selected as offering the best prospect of success, time intervals being measured by means of an oscillograph built in the Denver laboratory of the Bureau. This method was found very successful and arrangements were made to develop the method further by field measurements on the wasteway at Station 1146+30 of the main canal, Kittitas division, Yakima Project. Unfortunately, time did not permit the completion of equipment entirely suitable for field use.

Electrodes were fastened to the walls of the chute, spaced at different intervals; the electrodes were numbered, and these numbers are given in Col. 2, Table 9. Supplementing Mr. Hall's description, electrode No. 1 was 25 ft below the end of the tapered section; No. 10 was about 7 ft above the beginning of the vertical curve between the two steeper slopes; No. 11 was about 64 ft below the end of this curve; Nos. 15 and 16 were equidistant from the beginning of a circular curve of 134.6-ft radius and a central angle of 33° 06′; Nos. 17 and 18 were equidistant from the end of this curve; and No. 20 was at the lower end of the chute. The vertical curve between the two steeper slopes was a parabola with a horizontal length of 84 ft and a vertical drop of 34.87 ft. Distances between electrodes, measured along the slope, are given in Table 18. The salt

TABLE 18.—ELECTRODE SPACING (SEE TABLE 19)

Slope	CUMULATIVE DISTANCES, IN FEET, FROM THE FIRST ELECTRODE											
	0	10	40	50	100	110	200	210	300	310	400	410
	ELECTRODE NUMBERS											
0° 04′	17	18	19	20	...	...	...	...	...	...	...	...
10° 12′	1	2	...	...	3	4	5	6	7	8	9	10
33° 10′	11	12	...	...	13	14	15	16	...	...	...	...
Interval (ft)	0	10	30	10	50	10	90	10	90	10	90	10

used was of a fineness to pass a 140-mesh sieve. It was moistened until it could be molded by the hands into balls, which were dropped into the water a short distance upstream from the test reach. The size of the balls necessary was determined by trial and error and varied from 2 to 7 in.

The field trials were not wholly successful, partly because of inadequate apparatus and partly because of imperfect techniques. The separate velocity measurements obtained are given in Table 19. In determining times to obtain mean velocities, the distance between centers of gravity of the oscillograph diagrams should be used. However, the diagrams in many cases were indistinct and in some were not completely recorded. Sixteen of the clearest diagrams were chosen and the distances between the centers of gravity were computed. The ratios of the distances between the beginning points to the distances between the centers of gravity (ratio of maximum to mean velocity)

averaged 1.40. The ratios of the distances determined by averaging the beginning points with other characteristic points to the distances between centers of gravity averaged 1.24; when the distances between centers of gravity could not be determined satisfactorily, the distances thus obtained were divided by 1.24 and the result used to determine the velocities given in Table 19. The velocities given by Table 9 are averages of those given in Table 19.

TABLE 19.—VELOCITIES, IN FEET PER SECOND, BETWEEN THE ELECTRODES INDICATED (SEE TABLE 18)

Discharge (cu ft per sec)	(a) SLOPE, 0° 04′			(b) SLOPE, 10° 12′									(c) SLOPE, 33° 10′						
	E[a]	Velocity	E[a]	E[a]	Velocity	E[a]	Velocity	E[a]	Velocity	E[a]	Velocity	E[a]	E[a]	Velocity	E[a]	Velocity	E[a]	Velocity	E[a]
89	..	...	..	1	28.6	3	27.4	5	..	..	..	..	..	...	..	..	..	...	..
	..	...	..	..	...	3	27.4	5	...	..	...	..	..	...	..	...	..	...	..
	..	...	..	1	30.5	3	29.8	5	31.6	9	...	..	..	...	12	44.5	14	53.3	16
193	..	...	..	..	...	..	...	5	30.5	7	...	..	..	...	12	41.5	14	54.0	15
	..	...	..	..	...	..	...	..	...	..	...	..	..	...	12	56.1	14	...	..
	18	34.0	20	1	37.2	3	40.5	5	41.4	8	49.2	9	11	63.5	14	56.4	15	...	..
362	18	35.5	20	1	43.1	3	44.1	5	38.3	8	49.8	9	11	54.7	14	56.7	15	...	..
	18	34.0	20	..	...	..	...	..	...	..	...	..	..	...	12	57.3	14	52.7	16
	..	...	..	1	37.7	3	42.8	5	...	..	...	..	11	60.1	13	65.3	16	...	..
401	..	...	..	1	37.4	3	41.4	5	...	..	...	..	11	60.0	13	63.5	16	...	..
	..	...	..	..	...	..	...	..	...	..	...	..	..	...	13	58.5	16	...	..
	17	59.5	20	1	42.1	3	39.2	5	43.8	8	...	..	..	...	12	59.0	13	67.5	15
491	17	48.1	20	..	...	3	45.1	5	45.5	8	...	..	11	63.8	13	64.3	15	...	..
	18	45.4	20	..	...	3	41.4	5	46.2	9	...	..	..	...	12	62.1	14	61.5	16
	17	33.7	19	1	44.5	3	44.1	5	46.4	7	50.6	9	11	67.5	13	64.0	15	...	..
587	17	45.3	20	..	...	3	47.0	5	49.2	7	53.1	9	11	63.7	13	66.7	15	...	..
	17	60.4	20	..	...	..	...	5	46.2	7	51.6	9	11	60.4	13	67.5	16	...	..
	..	...	..	..	...	..	...	..	...	..	...	..	..	...	13	69.5	15	...	..
	17	67.6	20	1	46.6	3	46.5	5	41.3	7	56.0	9	11	77.2	13	75.2	16	...	..
719	17	60.0	20	1	46.5	3	45.3	5	50.0	7	54.3	9	11	71.1	13	65.1	16	...	..
	17	57.5	19	..	...	..	...	5	56.5	7	51.3	9	..	...	13	66.3	15	...	..
777	17	57.5	19	1	52.1	3	50.1	5	50.9	7	50.8	9	11	65.9	13	94.0	16	...	..
	17	50.6	19	..	...	3	51.7	5	...	7	51.9	9	..	...	13	85.6	15	...	..
	17	58.6	20	1	58.6	3	60.1	5	53.3	7	54.8	9	..	...	12	68.7	13	75.2	15
922	18	56.0	20	1	49.2	3	52.3	5	48.6	7	55.4	9	..	...	12	77.6	13	74.5	15
	..	...	..	..	...	3	49.0	5	...	7	52.2	9	..	...	12	70.5	14	68.6	16
	17	62.3	20	1	58.2	3	51.1	5	58.3	7	54.0	9	..	...	12	91.1	15	...	..
	18	55.4	20	1	59.8	3	57.5	5	55.5	7	51.2	9	..	...	12	75.1	15	...	..
1,005	..	...	..	..	...	..	...	5	53.8	7	54.1	9	..	...	..	...	13	81.6	15
	..	...	..	..	...	..	...	..	...	..	...	..	..	...	..	...	13	82.6	15
	..	...	..	..	...	..	...	..	...	..	...	..	..	...	..	...	14	74.1	16

[a] E denotes "Electrode Number," marking the electrode numbers between which velocities were observed. The position of the electrodes is given in Table 18.

Water-surface profiles at the midpoint of each 10-ft electrode station were measured by point gage at points 1 ft apart. Readings on the floor were made at the same points by the same gage after completion of the tests. The differences were taken as the depths. The water surface was considered to be at the base of the loosely flying spray. Because of the difficulty of visual observation, the vibration of the gage was relied on in making the measurements. There was an easily recognized feel to the gage when it had penetrated this spray and encountered the surface beneath. This surface included many small waves and rollers and the values of depth used were approximate means between crests and valleys. Areas given in Table 9 are averages of the areas in the reach.

Discharges were measured simultaneously with the tests by current meter in the canal above the wasteway. For a few tests, some water passed the inlet for irrigation purposes, and this also was measured by current meter.

The memoranda from which the experimental data were taken contained a mathematical analysis by V. L. Streeter, Assoc. M. Am. Soc. C. E., to which Mr. Hall's is so remarkably similar that it appears one more example may be added to the classic list of cases in which independent investigations arrived at the same result. Mr. Streeter's analysis ended with Eq. 17, and to this point Mr. Hall's equations and illustrations are practically identical with his except that Eqs. 8, 9*a*, and 9*b* have been added and changes made in the two following respects.

(1) Mr. Streeter's form of Eq. 5 was

$$f = K\,V^2 \quad \text{.........................}(72)$$

from which it follows that the normal Chézy formula must be modified if observed quantities are used. Mr. Hall puts the term ρ in the right-hand member of this equation and "proves" exactly the opposite. Evidently Eq. 5 must express a physical fact rather than a mathematical assumption. Eq. 4 expresses the simple fact that, for uniform motion, the force producing and that opposing motion are equal. The former is the component of gravity acting parallel to the slope; the latter is the friction along the wetted perimeter. When an increase in depth is produced by adding air, the force producing motion remains substantially the same, and the area over which the opposing force acts increases. Unless, therefore, the unit force opposing motion is decreased faster by the addition of air than the perimeter is increased, velocities would be reduced.

It has been shown[45] that the mean velocity in an open channel is independent of, or inversely proportional to, the kinematic viscosity. The kinematic viscosity of air, at 60° F, is about thirteen times that of water; hence, there is little reason to expect that velocities would be increased by addition of air. One would expect, rather, a reduced velocity, and, therefore, a higher value of n. Mr. Hall seems to recognize this, but, having concluded that the normal coefficient should be used for the aerated section, then proposes to use a smaller value of n for water alone, thus assuming that resistance at high velocities is less than at low ones—a strange conclusion.

(2) Mr. Streeter introduced an additional term, $\frac{K\,V^2}{R^p}$, in the right-hand member of Eq. 14 to allow for air resistance at the contact "surface" of air and water. This is referred to in the text following Eq. 21. That such a resistance exists should be obvious. There is nothing "fictitious" about it; nor is it "* * * based on the area occupied presumably by the water and not by the air." The extent to which empirical formulas allow for this resistance and what further allowance, if any, is required, are other matters. Values of 3.64×10^{-5} for K and 1.46 for p were deduced. However, present data on velocities afford little basis for evaluation of such a correction.

Mr. Hall refers to Eq. 17 as the "* * * basic equation for flow in steep rectangular chutes * * *." Being simply a mathematical combination of the Manning formula and Bernoulli's equation, it will apply to any rectangular

[45] "Laws of Turbulent Flow in Open Channels," by Garbis H. Keulegan, *Research Paper RP*1151, National Bureau of Standards.

channel, steep or otherwise. Aside from its possible mathematical elegance, it seems to possess no advantage over the familiar form

$$l = \frac{\Delta E}{\Delta S} \qquad (73)$$

in which ΔE is the change in energy (normal depth times cos θ plus velocity head) and ΔS is the difference between the average friction slope over the length l, determined from the Manning, or other, formula and the bottom slope (sin θ). For cases to which Eq. 17 applies, Eq. 73 may be solved directly for l by assuming values of the depth. Otherwise, it is Eq. 27 in simple form and is easily solved by trial and error.

At the risk of inviting the obvious rejoinder, the writer confesses to some bewilderment at the multiplicity of formulas and of values of n; and as to just how the formulas are to be used. For design purposes, there are no observed values of n, v, y, or R. In his "Conclusions," Mr. Hall recommends use of the "computed area, assuming no air entrainment," which "necessitates the use of a value of n smaller than that normally used, and one that depends for its value on the velocity of the water, as indicated by Eq. 26." In Eq. 26 the value of n_c depends on V and on K. How V is to be determined is not clear. Values of K vary from 0.00535 to 0.0104, furnishing quite a range of choice. For methods of determining values of n_c when K_1 is not zero or when the channel is not rectangular with parallel sides, the reader seems to be left to his own devices, or to the use of Eq. 8, which requires that the observed hydraulic radius be known.

There is a velocity below which self-aeration does not take place; from Table 20, its value seems to lie between 10 and 40 ft per sec. In Eq. 24*a*, the constant K_1 should have a value corresponding to this velocity. The plotted points on Mr. Hall's charts do not warrant making the value of K_1 equal to zero. All formulas proposed indicate that, for steady flow, a stable mixture would exist. If this is true, entrance conditions would exercise only a local effect, which could not be included in a general formula.

The experience of the Bureau does not indicate that such confidence could be placed in Mr. Hall's velocity measurements. Due to entrained air the surface of water at high velocities appears very white. The spray above the surface makes it hard to detect the color or the boundaries of the colored mass. The estimation by eye of the position of the center of the colored mass is very uncertain. The reaction time of an individual introduces a time lag in the use of the gun and stop watch, which, for short distances, could result in an appreciable error. For greater distances, this error might be negligible, but the color cloud becomes indistinct and lengthens, making the estimation of the position of its center harder. Because of surface conditions the body of the fluid cannot be seen and the only visible color naturally would be on the surface. Even if errors of measurement could be avoided, the relation between surface and mean velocities is unknown. In short distances the sudden changes in observed velocities, without corresponding depth changes, indicate inaccurate measurements. For example, in Table 3 the decrease in velocity head

TABLE 20.—Compilation of Earlier Chute Measurements

No.	Description	Width (ft)	Sin θ	n[a]	Q (cu ft per sec)	Depth d (ft)	Hydraulic radius (ft)	V_1 (ft per sec)	$\frac{Q}{A\ V}$ (%)
	(a) Ehrenberger's Model (Wood)[b]								
1	Chute I	0.82	0.153	0.0071	0.353	0.057	0.050	9.61	79.0
				0.0076	0.706	0.090	0.074	11.94	79.0
				0.0081	1.09	0.123	0.094	13.29	80.7
				0.0086	1.57	0.161	0.116	14.53	80.1
2	Chute II	0.82	0.202	0.0074	0.353	0.054	0.048	10.24	77.0
				0.0080	0.706	0.087	0.072	13.06	75.0
				0.0077	1.09	0.120	0.093	14.70	75.0
				0.0085	1.57	0.156	0.113	16.04	75.8
3	Chute III	0.82	0.305	0.0076	0.353	0.051	0.045	11.48	72.9
				0.0079	0.706	0.082	0.068	14.47	71.6
				0.0084	1.09	0.115	0.090	16.31	70.4
				0.0091	1.57	0.151	0.111	17.55	70.7
4	Chute IV	0.82	0.444	0.0073	0.353	0.048	0.043	13.22	67.5
				0.0075	0.706	0.079	0.066	16.80	64.4
				0.0081	1.09	0.112	0.088	18.73	63.1
				0.0089	1.57	0.148	0.109	20.18	63.5
5	Chute V	0.82	0.606	0.0064	0.353	0.052	0.047	15.78	51.3
				0.0069	0.706	0.085	0.071	19.46	51.2
				0.0074	1.09	0.118	0.092	21.85	51.0
				0.0081	1.57	0.156	0.113	23.62	51.3
	(b) Ruetz Wasteway (Wood)								
6	Trapezoidal[b c]	8.2[d]	0.606	0.0098	17.7	0.10	0.097	23.6	64
				0.0080	28.3	0.16	0.157	32.2	44
				0.0078	120.0	0.49	0.446	56.1	39
7	Trapezoidal[b]	8.2[d]	0.606	0.0067	27.9	...	0.210	35.5	41
				0.0064	36.0	...	0.272	40.8	38
				0.0063	52.3	...	0.361	47.2	37
				0.0078	129.2	...	0.512	57.0	40
8	Trapezoidal[b]	8.2[d]	0.606	0.0062	32.8	0.26	0.249	39.6	37
				0.0064	56.6	0.38	0.348	48.2	37
				0.0069	62.5	0.39	0.364	47.9	37
				0.0066	70.3	0.45	0.407	51.2	38
				0.0076	151.9	0.74	0.640	62.3	43
9	Trapezoidal[e]	....	....	0.0026	36	0.31	0.29	68.6	20
				0.0036	52	0.36	0.33	65.9	25
				0.0054	56	0.46	0.42	53.5	27
				0.0047	98	0.45	0.41	70.2	39
				0.0058	132	0.84	0.72	70.2	25
				0.0066	159	0.94	0.79	70.2	27
				0.0072	191	...	....	70.2	64
	(c) Dago[e]								
10	....	3.28	0.246	0.0113	49.1	0.64	0.46	33.1	64
				0.0102	26.1	0.43	0.34	28.2	66
	(d) Benkok[e]								
11	....	3.28	0.602	0.0101	211.9	2.20	0.94	77.1	38
				0.0087	102.4	1.21	0.70	67.2	38
	(e) Mallnitz[e] (Trapezoidal)								
12	Trapezoidal	6.56	0.408	0.0066	166.0	1.23	0.92	68.6	....
				0.0067	134.2	1.15	0.88	63.0	....
	(f) Fargo Drop[f] (Wood)								
13	....	6.0	0.124	0.0091	95.0	0.57	0.48	32.3	86

TABLE 20—*Continued*

No.	Description	Width (ft)	Sin θ	n[a]	Q (cu ft per sec)	Depth d (ft)	Hydraulic radius (ft)	V_1 (ft per sec)	$\frac{Q}{A\ V}$ (%)
	(*g*) MORA WASTEWAY[f] (CONCRETE)								
14	....	5.0	0.081	0.0072	27.5	0.32	0.28	22.0	78
	(*h*) VALLEY MOUND CHUTE[f] (CONCRETE)								
15	....	5.0	0.156	0.0066	22.4	0.27	0.22	26.3	63
	(*i*) ARENA CHUTE[f] (CONCRETE)								
16	Section 1	6.0	0.202	0.0104	23.26	0.24	0.22	20.4	79
17	Section 2	6.0	0.151	0.0096	23.26	0.22	0.21	19.6	90
18	....	6.0	0.206	0.0094	50.40	0.32	0.29	29.4	89
	(*j*) LIZARD CHUTE[f] (CONCRETE)								
19	Chute No. 1	3.0	0.082	....	0.44	0.04	0.04	4.97	73
				....	2.04	0.11	0.10	9.33	66
				....	2.80	0.12	0.11	10.9	71
				0.0073	5.72	0.17	0.16	14.3	72
				0.0091	7.97	0.24	0.21	14.2	78
				0.0085	16.42	0.36	0.29	19.2	79
20	Chute No. 2	3.0	0.194	....	0.44	0.05	0.05	6.9	42
				....	2.04	0.10	0.09	7.8	87
				....	2.80	0.10	0.10	11.1	85
				0.0067	5.72	0.16	0.14	19.8	60
				0.0115	7.97	0.21	0.18	16.0	80
				0.0095	16.42	0.31	0.26	23.8	78
	(*k*) KITTITAS WASTEWAY[f] (CONCRETE; MEASURED DOWNSTREAM FROM CHUTE)								
21	....	8.0	0.547	0.0130	231	0.80	0.67	52.2	69
				0.0137	484	1.3	0.98	66.0	70.5
	(*l*) ALBERTA, CANADA[g] (CANADIAN PACIFIC RAILWAY)								
22	Hammerhill Flume (Metal)	5.10	0.057	0.0082	61.0	....	0.573	27.5	85
				0.0102	26.5	....	0.433	18.6	88
23	Dalroy Flume	10.2	0.032	0.0125	59.2	....	0.993	15.2	54
				0.0155	151.4	....	1.311	17.6	76
24	Lateral C-11 Flume	0.9	0.052	....	1.24	....	1.0107	9.82	99
25	Secondary Canal	4.18	0.025	....	6.16	....	0.155	9.25	95
	(*m*) SOUTH CANAL CHUTE,[f] MILEPOST 2, UNCOMPAHGRE PROJECT (CONCRETE; TRAPEZOIDAL)								
26	Section 1; sides ½ : 1	8.84[d]	0.275	0.0175	382	1.15	0.98	40.24	87
				0.0211	465	1.30	1.07	38.17	99
27	Section 2; sides ¼ : 1	8.47[d]	0.070	0.0129	267	1.06	0.87	28.13	103
				0.0131	400	1.39	1.09	31.93	102
				0.0136	463	1.53	1.17	32.58	104

[a] Value required to give the observed velocity for a given slope and discharge. [b] See footnote 46. [c] Side slopes 1.5 on 1; adjusted values of d and V as given by Ehrenberger. [d] Bottom width of trapezoid. "Kolkabwehr und Stauraumverlandung," by A. Schoklitsch, Julius Springer, 1935. [f] Bureau of Reclamation experiments. [g] "The Flow of Water in Flumes," by Fred C. Scobey, *Bulletin No. 393*, U.S.D.A., 1933.

between Stations 3+75 and 4+00 for a discharge of 395 cu ft per sec is 22 ft, with a decrease in depth of 0.02 ft.

The depth measurement by means of staff gages on the sides of the channel gives the depth more or less to the top of the spray as the gage cannot be seen much below this surface. Just below this surface the percentage of air is much higher than at greater depths. At Kittitas the depth at each side of the chute for a distance of about 1 ft from the walls was appreciably greater than in the center, the difference amounting to as much as 0.8 ft on a practically symmetrical section. The variation of the side depth from the mean amounted in some cases to more than 10%. The same effect has been reported by others.[46] Therefore, the Kittitas data are probably not on the same basis as those of Mr. Hall.

Mr. Hall states in his "Conclusions" that "the retardation factors n in the Manning or the Kutter formula, obtained by using observed velocities and areas, agree with those obtained in normal channel flow in similar materials. This relation is demonstrated to be correct both in theory and from actual observations." The theory already has been questioned. Neither do "actual observations" fully support this statement. Data found in literature on the subject are given in Table 20. Inconsistencies therein are evident, particularly in the case of the Ruetz wasteway (Table 20(*b*)). On the assumption that the velocities given represent normal, or terminal, velocities, the writer has computed values of n in the Manning formula for the given values of discharge and slope without regard to air entrainment, with the results shown in Table 20. These are comparable to the values of n_c in Table 12.

Many of the values are lower than those which normally might be expected. This could result from too high a velocity or, to a less extent, from too small a discharge. In some cases, the method of measuring velocities is unknown. In others it was measured by means of floats. A surface float traveling in the center of the chute, where it naturally would be drawn, very likely might give a velocity higher than the mean. In the writer's opinion, the most dependable measurements of velocities are those on Kittitas wasteway, given in the paper, and those on Ehrenberger's model, given in Table 20(*a*). In both of these cases and in several others the values of n are in the normal range. In other cases, in view of the very fair agreement of the Manning formula with the rational formula derived by G. H. Keulegan,[45] there is more reason to suspect the velocity or the discharge measurements than to suppose that higher velocities would result in such radical changes in usual values of n, especially when the change is a decrease. As is well known, water at shallow depth on steep slopes tends to travel in waves which overrun each other. Such waves normally would exist in many of the experiments given in Table 20, and it well may be doubted whether a satisfactory determination of mean velocity could be made under such conditions. The data in Table 20 show a tendency toward smaller values of n for shallow depths.

Obtaining the velocity, unfortunately, is only a part of the problem, as the depth in some cases is of equal or greater importance. In self-aerated flow the

[46] "Flow of Water in Steep Chutes with Special Reference to Self-Aeration," by R. Ehrenberger, *Osterreichischen Ingenieur und Architecktenvereines*, Nos. 15/16 and 17/18, 1926.

air must enter at the contact between air and water. As a result of the air resistance, this contact is not a definite surface, but a zone in which the percentage of air in the mixture changes from practically 100% to an unknown percentage. Aeration of water not in intimate contact with the air may be caused by air bubbles carried down by turbulence, the air having to pass through this zone into which the turbulence may not extend. Such bubbles must be carried down against their natural buoyancy, which suggests study along the lines of sedimentation studies. It would seem plausible to suppose that the percentage of air might decrease with depth on account of the tendency of the bubbles to rise or of their decrease in size due to increase in pressure. It might be assumed, therefore, that only a limited depth would be affected in deep water; also, that the depth so affected would be a function of time. Such tendencies were noted by Ehrenberger.[46]

It would appear that a formula for aeration should allow for channel roughness. For example, an unlined and a lined channel could have the same Froude number, but whether the air entrainment would be the same remains a matter of speculation. It has been shown that K_1 in Eq. 24*a* should not be zero. If it is not, complications arise; one would conclude from Eq. 24*a*, using constants for the upper Kittitas chute, that no air would be entrained by a velocity of 57 ft per sec if R is 2.5 ft. The case is not unusual; the conclusion doubtful. The not-too-good fit of the plotted data with Mr. Hall's formulas may be due as much to the unfortunate fact that all experiments were made on closely similar channels as to any validity of his use of the Froude number.

It has been suggested that the increase in depth is a function of some power of the velocity, and there seem to be some hypothetical grounds for such an assumption.

The term "depth" should be defined. An accurate method of measuring velocities is badly needed, and, until one is developed, little progress is likely. A study of the mechanical process by which the air is entrained should be helpful. Possibilities of laboratory research, and perhaps of theoretical investigation, may not have been exhausted. Until reliable information to the contrary can be had, velocities as determined by the usual methods with the ordinary values of n can be regarded as being reasonably satisfactory; but formulas for determination of the quantity of entrained air should be considered as empirical and used with caution. With the data at hand, it should be possible to prepare conservative designs for small structures of the type and size of those used for the experiments. Whether equally satisfactory results can be expected for larger structures, only further information will tell.

T. J. Corwin, Jr.,[47] M. Am. Soc. C. E.[47a]—Comparatively little information on water flowing at high velocities in chutes is available to the designing hydraulic engineer. For this reason Mr. Hall's paper is a very valuable contribution.

It is doubtful if the bulking that results from the entrainment of air was anticipated when designs were prepared for the Rapid Flume, South Canal

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[47] Asst. Engr., Pacific Gas & Elec. Co., San Francisco, Calif.

[47a] Received by the Secretary April 24, 1943.

chute, or Hat Creek No. 2 chute twenty-five to forty years ago. Design computations for the Rapid Flume could not be located, but a velocity of 32 ft per sec was used in designing a baffle box which was reconstructed at the end of the chute in 1940. This value substantially checks the observed velocity of 33.3 ft per sec.

For the South Canal chute the computed and observed velocities can be compared in Table 21. In the Hat Creek No. 2 chute (Table 21) the observed

TABLE 21.—COMPARISON OF COMPUTED[a] AND OBSERVED VELOCITIES (FEET PER SECOND)

(a) SOUTH CANAL CHUTE								(b) HAT CREEK No. 2 CHUTE			
Q = 400 vs. Q = 363				Q = 400 vs. Q = 334				Q = 900 vs. Q = 395			
Station	Computed	Observed	Water (%)	Station	Computed	Observed	Water (%)	Station	Computed	Observed	Water (%)
0+50	26.0	28.2	76.1	17+00	14.5	23.2	91.2	0+00	25.0	28.5	61.5
8+00	23.5	27.6	78.3	23+00	14.5	16.6	92.8	1+25	57.5	60.5	65.8
14+00	19.0	26.4	74.9	25+00	21.2	21.7	100.0	2+25	66.3	73.9	53.8
....	....	....	....	26+25	30.7	33.5	70.6	2+75	67.5	75.9	51.0
....	....	....	....	32+00	20.5	25.5	85.6	3+75	74.0	72.3	50.0

[a] In the computed values (Q = 400 and 900 cu ft per sec) n = 0.015 was used.

velocities are higher than the computed values. The chute was designed for a capacity of 900 cu ft per sec with $C = 100$ or n = about 0.015. No allowance was made for air entrainment. The freeboard allowance was rather limited for such high velocities. The test indicates that, because of the bulking effect of the air, the capacity of the chute is roughly only 50% of the design capacity. The original wooden flume has been replaced with a concrete flume of 600 cu ft per sec capacity. The chute may carry this flow without serious splashing at Station 2+75 (see Table 21).

With regard to "Theory of Flow in Chutes," the three premises stated should be in accord with the desires of all designing engineers. Requirement No. 3 is especially true since the first and most important part of the design is to carry the full flow regardless of any number of reasons as to why the capacity is inadequate.

From actual experience with model tests, one can concur readily with the statement inferred in the "Synopsis" that it is only possible to observe the phenomenon on the prototype.

In designing conduits with horizontal curves and relatively high velocities, ample freeboard should be provided because the actual rise in the water surface on the outside of curves has been observed at about twice the theoretical value. The paper brings out the effect of cavitation on vertical curves when the actual velocity is in excess of the computed velocity and the stream leaves the bottom of the chute. Vertical curves should be designed for velocities computed on the basis of absolute minimum friction losses.

The most difficult problem in connection with the chutes is to develop a suitable transition so the water can enter without entrainment of air or surface

waves. The ground topography at spillways, which are placed along the side of bench flumes, makes it necessary for the designer to accept the turbulent condition at the entrance and to provide ample freeboard.

It is fortunate that the wooden Rapid Flume was available for test so that a comparison could be obtained with concrete chutes. Because of the smooth condition of the surfaced timber, the percentage of entrained air at equal velocities was found to be less than for concrete surfaces.

Mr. Hall has been able to draw the conclusion from these tests that the air bulking was reduced with larger cross sections and greater volumes of discharge. This is particularly fortunate; otherwise, many spill chutes at dams might be inadequate for full flood discharge.

In the future one may design chutes with more confidence, knowing what allowance to make for bulking due to air and also that the normal values of n are independent of magnitude of the velocities.

Corrections for *Transactions:* In April, 1943, *Proceedings*, page 517, Fig. 23, change abscissa caption to read "Values of $\frac{V^2}{g\,R}$," omitting = Froude's number; and, on page 518, line 14, change "$\frac{V}{g\,R_c}$" to "$\frac{V^2}{g\,R_c}$".

AMERICAN SOCIETY OF CIVIL ENGINEERS
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DISCUSSIONS

PHYSICAL PROPERTIES THAT AFFECT THE BEHAVIOR OF STRUCTURAL MEMBERS

Discussion

By Messrs. Almon H. Fuller, and C. F. Goodrich

Almon H. Fuller,[21] M. Am. Soc. C. E.[21a]—Matters of much interest to the designing engineer are dealt with in this paper although the full implications of them may be outside of his experience. It raises some questions, frequently overlooked, which need to be considered in revising specifications and in the designing of unusual structures.

The author states, in "Summary and Conclusions" (2), that "* * * Additional information is needed relative to the susceptibility of structural steels to age embrittlement subsequent to plastic deformation." The writer would expect age embrittlement to follow oft-repeated plastic deformations but not to follow the occasional overstress which results from fabrication and erection procedures, secondary stresses, and deformation stresses in general. As the effect of such overstresses is to produce a slight change in the material and to raise the elastic limit, it seems likely that, in many a case, a repetition of the condition which produced the overstress might keep the unit stresses within the raised elastic limit. When elastic stability was not established after the first overstress, it might be established in such a small number of overstresses as to restore elastic behavior without danger of embrittlement. Pending further researches on the subject it may be possible to make pertinent observations of existing conditions.

In the paper on "Theory of Limit Design,"[22] by J. A. Van den Broek, M. Am. Soc. C. E., as well as in the one under consideration, questions are raised and helpfully discussed which demand thoughtful consideration for those situations for which close designing is essential. The effect of both papers should be to allay and not to create any fear that material will not adjust itself

Note.—This paper by Wilbur M. Wilson, M. Am. Soc. C. E., was published in December, 1942, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: March, 1943, by Jonathan Jones, M. Am. Soc. C. E.; and May, 1943, by A. B. Kinzel, Esq.

[21] Prof., Civ. Eng., Iowa State College, Ames, Iowa.

[21a] Received by the Secretary April 15, 1943.

[22] "Theory of Limit Design," by J. A. Van den Broek, *Transactions*, Am. Soc. C. E., Vol. 105 (1940), p. 638.

to apparently unfavorable conditions wherever the design has been made intelligently.

Can it be possible that, for structural steel, some of the adjustment (perhaps plastic flow) really takes place within the range of stress called yield point, elastic limit, and even proportional limit? Concrete is accepted as imperfectly elastic, and therefore flow is recognized well within accepted working stresses. Steel is usually regarded as perfectly elastic although the physicists are doubtless correct when they assert that it is not. Although the plastic flow (or creep) at normal temperatures is too slight to be measured within the precision of the usual deformation apparatus within the time of laboratory readings, two publications[23,24] record deformations over several weeks' time which are clearly greater than would occur within an hour or so for the same unit stresses. In his "Synopsis," the author expresses one purpose of the paper "* * * to raise the question as to whether or not there are other characteristics of the material [steel] that should be considered." Not even the flow of concrete is great enough to make undesirable changes in the dimensions of a structure; but quite likely such flow does relieve local stress concentrations and thereby does contribute to satisfactory functioning of the structure as a whole. Is it not likely that the very slight plastic yielding of steel, within working stresses, also contributes to the best functioning of a steel structure?

C. F. Goodrich,[25] M. Am. Soc. C. E.[25a]—The author justifies the practice of reinforcing structural columns by attaching the reinforcement with longitudinal welds, even though the thermal stresses due to the welding are equal to the yield point of the material. He is careful, however, to give approval only to the idea of retaining such old structures in service as must be reinforced to carry safely increased loads.

The simplicity of welding reinforcing material on old structures, as compared to riveting, makes welding the ideal solution of the problem of strengthening overloaded bridges. Tests have shown that the high thermal stresses resulting from such practice do not necessarily impair the load-carrying capacity of the reinforced structure.

A somewhat different problem is presented when two different steels with different physical properties are used for one new member. Long riveted plate girders have been built with a carbon-steel web plate and silicon-steel flanges. When designing the web plate to resist buckling, it is found that the shearing capacity of a silicon plate is not fully utilized and a carbon-steel plate is often more economical. Many engineers have questioned this practice because the carbon-steel web will necessarily be stressed the same amount as the silicon-steel flange, in which a working stress of 24,000 lb per sq in. is usually specified. Thus the determination of the capacity rating of such a girder presents a nice problem. A capacity stress for carbon steel of 0.8 times the 33,000 lb per sq in. yield point is 26,400 lb per sq in., which is only 10%

[23] *Bulletin No. 72*, Eng. Experiment Station, Iowa State College, Ames, 1924.

[24] *Bulletin No. 40*, Eng. Experiment Station, Ohio State Univ., Columbus, 1928.

[25] Chf. Engr., Am. Bridge Co., Pittsburgh, Pa.

[25a] Received by the Secretary April 20, 1943.

more than working stress of the silicon flange; whereas a capacity stress for silicon steel of 0.8 times the 45,000 lb per sq in. yield point is 36,000 lb per sq in., which is above the yield point of the carbon web.

In the accepted theory of girder design, the web is assumed to take its proportion of the bending moment and will doubtless do so in a well-designed, well-built girder of carbon web and silicon flange under static loads up to the yield point of the carbon web. The expectation that the web will continue to take its proportionate part of the bending moment beyond this point or under repeated loads somewhat below this point would seem to be in the realm of wishful thinking.

The part of the web between the compression flange angles probably is locked there so that it cannot yield in compression because it will have no place to go. This hardly seems true, however, of the part of the web between the tension flange angles, or just outside of them, where necking down (which accompanies plastic yielding in tension) is not restrained by the clamping action of the flange angles, especially under repeated loads and the possible loosening of rivets. It seems more than probable that the carbon web, under these conditions, no longer could be relied upon to take its proportionate part of such overload. Thus, the silicon flange would have to take all of it. It is probably true that the silicon flange will continue to act elastically up to its yield point, provided rivets have not become loose and the girder is otherwise integral.

The question might well be asked: Would responsible engineers evaluate a girder with silicon flanges and carbon web to carry future overloading and to resist impact and fatigue as highly as they would a girder of the same makeup but with silicon web? The answer is probably negative. The entire matter then resolves itself into whether the engineer is willing to accept this loss of load-carrying capacity in order to effect the undoubted first cost economy of using the carbon web.

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DISCUSSIONS

SEISMIC SUBSURFACE EXPLORATION ON THE ST. LAWRENCE RIVER PROJECT

Discussion

By Messrs. Arthur J. Grier, F. W. Lee, L. Don Leet, and William L. Shannon and Winthrop A. Wells

Arthur J. Grier,[6] M. Am. Soc. C. E.[6a]—Considering how much has been written about geophysical exploration in the fifteen years since 1928, civil engineering literature is notably barren of references to the subject. It is difficult to explain why this laboratory technique, applied in the wide-open spaces, has had so little consideration by civil engineers and why they have made so few contributions to its development.

Perhaps, too often, successfully applied geophysics has been publicized without due emphasis on the advisability of confirming favorable implications by more common and costly exploratory methods before undertaking large expenditures. Geophysics is essentially a technique of elimination by comparatively inexpensive methods to assure the largest amount of information at the least cost in time and money. Its results are highly indicative but should not be accepted as determinative. It is a most useful tool in the hands of those who hold that engineering is not averse to making a dollar earn the most interest.

The authors frankly state that the seismic program of the Corps of Engineers was intended to guide and augment the drilling program and that all was to be interpreted by the observed geology of the region. The physical data of the shock wave were to be correlated with and confirmed by the more common physical tests of applied optics and mechanics. This sequence is just as profitable in civil engineering as it is in mining.

Seismic geophysics is specially applicable to the study of slightly dipping formations such as those for foundations and oil structures. Steeply dipping contacts of quartz veins, dikes, and faults, found in mining and water supply problems, may be delineated rapidly and accurately by electrical methods based on a difference of resistance.

Note.—This paper by E. R. Shepard, Esq., and Reuben M. Haines, Assoc. M. Am. Soc. C. E., was published in December, 1942, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: April, 1943, by R. Woodward Moore, Esq.

[6] Cons. Engr., Geophysical Exploration, Oakland, Calif.

[6a] Received by the Secretary March 29, 1943.

Since 1936 the writer has used an instrument which is simple, sensitive, and portable, based on an electrical circuit which he devised and has never seen described elsewhere. The fundamental circuit without refinements and reversing switches is shown in Fig. 14. This method employs a unidirectional electric field set up by means of common B batteries and two metal current electrodes marked C_1 and C_2. Within this imposed field are three equally

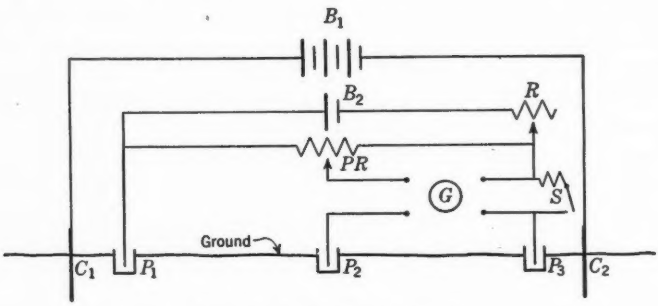

FIG. 14.—FUNDAMENTAL CIRCUIT

spaced nonpolarizing porous cups containing copper sulfate solution in which are the potential electrodes P_1, P_2, and P_3. These cups establish in the ground the two arms of a Wheatstone bridge, the other two arms being within the instrument and composed of the two segments of the potentiometer rheostat P_R with a dial graduated 0 to 100. The field is made strong enough that the potential at P_2 is intermediate between that at P_1 and P_3.

Without further provision a current would flow through these cups and the instrument, and the apparent resistance of the ground, in reality, would be the sum of the ground resistance plus the contact resistance at the potential electrodes, which might vary with each setup over the same ground. So it becomes necessary to employ the principle of the compensation bridge, or potentiometer, and neutralize the potential drop between P_1 and P_3 with an equivalent electromotive force from the battery B_2, adjusted by means of the rheostat R. Neutralization is indicated when the galvanometer G (which has been cut into that circuit) reads null, and since there then is no current through P_1 and P_3, there can be no contact resistance.

With the compensation bridge established, the Wheatstone bridge is effected by cutting the galvanometer over into the circuit P_2-P_R and closing the switch S with included resistance equal to that of the galvanometer. If the contactor is moved along the graduated meter wire P_R until the bridge is in balance, no current flows through the potential electrode P_2 and likewise there is no contact resistance at that point.

It remains only to read and note the two segments of the graduated dial P_R which accurately reflect the relative potential drop, and hence the relative ground resistance, of the two adjacent stations under consideration. There is but one instrument, the galvanometer, to be observed, and no mathematical calculations whatever are required.

The diagram does not show reversing switches, but it is necessary to reverse polarities and repeat the operation. The average of the readings will balance out the natural earth potential which usually will be encountered. That is the entire operation.

All electrodes, both current and potential, are moved ahead one station, and new readings taken as before. Upon the completion of the traverse, the notes plot a profile of relative earth resistance to a depth depending upon the spacing of the electrodes. Any anomalies become conspicuous, and such areas can be explored further by rerunning with different electrode spacing and by probing for depth at critical spots. It is observed that depth readings are much sharper when using the bridge circuit than when calculated and plotted in a curved line by the usual Wenner[7] formula. No attempt is made to determine specific earth resistance, and it is questionable if there is any advantage in doing so. Both methods are based on the same recognized laws pertaining to electric currents in a semi-infinite medium.

The entire subject of geophysics, including the several approved methods, may be regarded as an expansion into the realm of the unseen of the ancient science of surveying. It derives from a better knowledge of physical laws and is made possible by instruments of sensitivity and precision.

The economic value to civil engineering of this surveying of the unseen will be recognized as more field engineers with training in the physical sciences apply their skill to just such problems as the St. Lawrence River Project under discussion.

F. W. Lee,[8] Esq.[8a]—A careful explanation has been presented in this excellent paper of the technique as well as of the results that can be obtained by the application of seismic refraction methods for securing subsurface information regarding depth to bedrock. Little can be added other than to compliment the authors for the accurate manner in which they have executed this project.

Field observations made on Governors Island in the Boston (Mass.) Harbor have showed that, even under very simple geological conditions involving glacial drift, the speed in the drift is not uniform and that certain average values can be made to apply in each interval of observation. An attempt was made to chart such velocities in the glacial drift on the island.[9] Apparently, the ratio of gravel to clay in the drift is a factor. In water-sorted gravel beds, particularly where the individual stones all were worn into ellipsoidal bodies and deposited with their minor axes in a vertical position, the vertical and the transverse seismic velocities were not the same. However, the differences in boulder content overshadowed such observations.

In presenting this paper the Society has taken a very definite forward step in making surveys in three dimensions, combining parts under water with nonsubmersed areas. The good old transit which serves so well above ground has

[7] "A Method of Measuring Earth Resistivity," by F. Wenner, *Bulletin No. 258*, U. S. Bureau of Standards.

[8] Chf., Div. of Geophysical Exploration, Bureau of Mines, U. S. Dept. of the Interior, Baltimore, Md.

[8a] Received by the Secretary April 10, 1943.

[9] *Bulletin No. 8*, Dept. of Public Works, Boston, Mass.

added a brother for looking down into the earth, which at present has the title of seismic methods of exploration. Of these the refraction seismic method has proved particularly helpful for the measurement of depths to bedrock, as can be noted from Table 1, which shows that the seismic depth determinations agreed with the drill-hole record to within a few feet.

The following information is given as a matter of caution for executives who may be planning other applications of seismic refraction methods. A controlling feature of seismic methods depends upon the depth to which measurements are made, in conjunction with the accuracy of the final information expected. These various projects may be divided according to the depths of observation into the following classes:

Class	Approximate depths, in feet	Description
1	100	Depth to bedrock required for dam foundations, bridgeheads, road construction, water sources, and excavations.
2	400 to 500	Depths to preglacial channels, required for some foundation work and for sources of water supply for cities and regions.
3	800 to 1,000	Geological stratigraphy related to the planning of subsurface tunnels such as would be required for dewatering mines and mining areas.
4	3,000	Identification of large geologic bodies such as salt domes in southern Texas and Louisiana, which are of economic importance because of the oil generally found on the top and on the flanks of such domes.
5	$>$ 300,000	Depths to regions of change in magmatic material, useful for earthquake studies and in the detection of major geologic changes.

The criterion of a seismic analysis is the degree to which a seismic impulse may be traced from the shot point to the detectors for the proper speeds as well as for the distances traversed in each medium. Mathematically, the formulas for refraction seismic surveys are very similar to those for geometric optics. They differ from those for optics in that they are used to analyze the material traversed instead of to design special surfaces of lenses for certain critical characteristics.

Often, even for very simple field cases, the solution becomes complicated since there is generally a weathered zone near the ground surface which has a comparatively small seismic speed of the order of 1,000 ft per sec. Directly under this weathered zone is a much faster unweathered zone having a seismic speed of the order of 5,000 ft per sec. Below it are the various geological beds each having a characteristic velocity—in granite and dense limestones reaching velocities of the order of 20,000 ft per sec. Therefore small differences in the thicknesses of the slow beds can introduce large time errors which, when applied to the same ray path in the fast beds, will introduce large errors in depths. To these differences must be added corrections for elevations for the various

detectors on the shot line as well as for the variations of the topography of the bedrock. The cost of a project often depends to a large degree on the necessity of making supplementary seismic records for determining such irregularities singularly.

When making seismic surveys to a deeper horizon, the distances between the shot point and the detectors become increasingly greater, and the simple assumption that the segments of geologic beds over which the measurements are made may be considered as plane surfaces no longer can be maintained. The thickening and thinning of the intermediary beds make the analysis very slow and tedious. On encountering such difficulties, the field engineer making the seismic surveys uses overlapping and abutting traverse lines of observation for clearing out such singularities. After the field work is done, the results may be likened to a picture puzzle, each piece representing a seismic profile. These pieces must be fitted together into such an integrated whole as would be produced by the geological conditions in question. From this point the procedure becomes increasingly more complex with the addition of each intervening bed which must be taken into consideration. Also, the geological "quirks" which might be associated with each bed must be considered. In deep refraction surveys of the order of 1,000 ft, one often encounters as many as four or five characteristic beds, each of which must be defined and differentiated separately.

From the foregoing, it is evident that the path of a seismic survey engineer is a hard one. Nevertheless, such geophysical methods may be considered the first rays of a new branch in civil engineering for studying hitherto unsounded depths on a scale which may be called planet engineering. Upon the foundations established by such studies it is hoped that in the end mankind will be provided with a power of knowledge that will permit him to make this planet perform according to his will, in much the same manner as a violin in the hands of a master.

L. Don Leet,[10] Esq.[10a]—Considerable success in operations under sometimes rather difficult field conditions has been achieved by the authors. They are to be congratulated upon this application of the seismic method, which has established itself rather widely in recent years but is often anything but simple and straightforward in its execution.

In connection with the detailed discussion of methods perhaps it should be emphasized that the authors' system of shooting does not yield what are known as "reverse profiles." In other words, the observations along a line "ahead" of the detectors are based upon elastic waves which do not travel over a common path with those along the line "back" from the detector location. Structurally, the significance of this is that interpretations assume continuity and uniformity of the high velocity rock surface over a distance which represents the combined length of "ahead" and "back" profiles. In some regions such an assumption would lead to serious errors, but in the area covered by the authors' report this apparently was not the case.

[10] Associate Prof. of Geology, Harvard Univ.; and Seismologist in Charge, Harvard Seismograph Station, Cambridge, Mass.

[10a] Received by the Secretary April 30, 1943.

Perhaps it is a small point to mention in connection with so excellent a paper, but in the interests of accuracy there seems to be no harm in pointing out that the formulas by which depths were computed are credited to Messrs. Ewing, Crary, and Rutherford (1937),[3] as they have been on previous occasions by Mr. Shepard. Actually, those formulas had been in use for approximately ten years before publication of the paper by Mr. Ewing and his assistants, and it is not correct to imply that they were developed first in 1937. At least one place where they had been published in 1931 was in the Bulletin of the French Bureau of Mines.

WILLIAM L. SHANNON,[11] JUN. AM. SOC. C. E., AND WINTHROP A. WELLS,[12] ESQ.[12a]—In connection with preliminary foundation investigation for flood control structures in the Merrimack Valley watershed, the District Office of the Corps of Engineers, Boston, Mass., made use of the Shepard type of seismograph to determine the elevation of bedrock. During the autumn of 1938, the proposed Riverhill Dam site[13] was investigated extensively and some

TABLE 2.—COMPARISON OF SEISMIC INTERPRETATIONS WITH DRILLING RECORDS, MERRIMACK VALLEY WATERSHED

Location	Seismic line No.	Drill hole No.	Distance,[a] in ft	Seismic			Drill Hole		
				Ground elevation	Depth, in ft	Rock elevation	Rock elevation	Depth, in ft	Ground elevation
Riverhill Dam site	SS-23	D-1	20	422	>126	<296	<301.1	>121.0	422.1
	SS-12	D-8	95	425	62	363	362.4	73.8	436.2
Franklin Falls Dam	18		30	398	15	382.7			
	..	D-107	...	...	...	...	376.2	14.1	390.3
	19		30	384	20	364.7			
West Hopkinton Dam Site No. 1	110	D-4	130	365	82	283	<292.7	> 73.4	366.1
	52	D-12	110	362	78	284	277.0	85.0	362.0
	113	D-10	100	359	73	286	<283.1	> 75.0	358.1
Canal between W. Hopkinton and Everett Reservoir	142	D-14	0	415.5	8.6	406.9	409.9	5.6	415.5
	40	D-7	0	394	39	355	<377.7	> 20.0	397.7
	151	D-3	0.	418.3	28.9	389.4	<379.4	> 20.0	399.4
	159	D-4	0	404.0	23.3	380.7	<378.5	> 25.5	404.0
Everett Dam site	47	D-2	40	328.5	33.5	295	303.4	24.0	327.4
	84	D-7	20	407.8	10.8	397	384.3	13.3	397.6
West Peterboro Dam site	S-2	D-7	45	945	40	905	914.8	43.5	958.3
	S-1	D-5	190	945	90	855	<851.4	>100.0	951.4
	18	D-1	100	978	52	926	928.4	44.0	972.4
	S-7	D-11	60	985	28	957	943.8	23.2	967.0
	S-6	D-10	60	925	35	890	885.1	42.7	927.8
West Henniker Dam site	4	D-9	0	540	137	403	461.9	73.1	535.0
	7	D-2	15	612	154	458	<533.0	> 75.2	608.2

[a] Distance from drill hole to seismic center.

work was done at the Franklin Falls Dam site. In the summer of 1939, seventeen proposed dam sites were investigated by the seismic method. At some of these sites, at a later date, drill holes were located and drilled at or

[3] "Geophysical Studies in the Atlantic Coastal Plain," *Bulletin of the Geophysical Society of America*, Vol. 48, June 1, 1937, pp. 753–802; see also "The Seismic Method of Exploration Applied to Construction Projects," by E. R. Shepard, *The Military Engineer*, September-October, 1939.

[11] Associate Engr., U. S. Engr. Office, Boston, Mass.

[12] Asst. Engr., U. S. Engr. Office, Boston, Mass.

[12a] Received by the Secretary April 30, 1943.

[13] "Damsite Surveying by Seismograph," by Albert E. Wood, *Engineering News-Record*, March 28, 1940.

adjacent to seismic lines. The results of both seismic and drill hole data are summarized in Table 2. The method used in these investigations is similar to that described by the authors under the heading: "Determination of Overburden Depth by Two-Way Shooting."

The seismic investigations at each site immediately followed a thorough geologic reconnaissance. The results of the geologic reconnaissance and seismic data were then used as a basis for selection of sites for more detailed study. At three of the proposed sites, detailed explorations of some areas by seismograph were made for final explorations to supplement drilling and aid in exact location of structures.

In general, the agreement between seismic and drill-hole determinations, as shown in Table 2, is not as good as that obtained at the St. Lawrence River Project. It is believed that, to a large extent, the greater disagreement is due to the presence in this region of one or both of the following: (1) A variable, relatively high velocity weathered rock above sound rock; and (2) a highly irregular rock surface.

The writers agree with the conclusions presented by the authors. Particularly is it desired to emphasize the usefulness of the seismograph for preliminary investigations for dam sites on the basis of relative cost, time required, and accuracy when compared with drilling.

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DISCUSSIONS

AERATION OF SPILLWAYS

Discussion

BY J. C. STEVENS, M. AM. SOC. C. E.

J. C. STEVENS,[10] M. AM. SOC. C. E.[10a]—The data and formulas presented in this paper will aid greatly not only in designing spillways but in ascertaining the flow characteristics of existing spillways. The profession's thanks are due the author for his contribution.

The author apparently assumes that it is always desirable to prevent subatmospheric pressures beneath the nappe. For vertical-lift gates designed for major overflow heads, this is doubtless true in order to prevent vibration. With other types of gates or on overflow crests either without gates or with undershot gates, this may not be so apparent. For example, drum gates, such as those on the crest of Grand Coulee Dam or those on the spillway crest at Boulder Dam, may withstand the effects of considerable subatmospheric pressure without serious effect and with some resulting benefits of increased discharge.

On overflow spillways without crest gates, advantage should be taken of the fact that the efficiencies of such crests can be increased by not aerating the underface of the nappe, and it is that phase of the subject the writer wishes to discuss briefly.

At flood-control dams it has been the practice to design for great overflows under high heads. The cost of the spillway thus becomes a large part of the total developmental cost. Adding some portion of an atmospheric head to the water head, therefore, presents opportunities for more economic design. Of course, the increased load on the spillway section, resulting from subatmospheric pressures beneath the overflowing sheet, must be taken into account, but it will be found they need give little concern in the gravity type of overflow dams.

Suppose a crest profile is adopted that will be just at the limit of form to produce no subatmospheric pressures in the interface between nappe and con-

NOTE.—This paper by G. H. Hickox, M. Am. Soc. C. E., was published in December, 1942, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: March, 1943, by Joe W. Johnson, Assoc. M. Am. Soc. C. E., and May, 1943, by J. W. Howe, Assoc. M. Am. Soc. C. E., and Claud C. Lomax, Jr., Jun. Am. Soc. C. E.

[10] Cons. Hydr. Engr. (Stevens & Koon), Portland, Ore.

[10a] Received by the Secretary April 7, 1943.

crete for a given design head, H_d. Then if the head is increased to some greater value H_o, the discharge coefficient will also be increased because of the creation of subatmospheric pressures at the interface, and a consequent addition of some atmospheric head to the water head.

The magnitude of these increases was investigated[11,12] by Hunter Rouse, M. Am. Soc. C. E., and Lincoln Reid, Assoc. M. Am. Soc. C. E. It was shown that the design head might be increased several times with continually increasing coefficient before separation of the nappe. These increases check fairly well with the increases in discharge given in Fig. 10, as the following example will show.

The No. II profile of overflow crest of the Rouse-Reid experiments[13] was used. A design head of 6.0 ft was selected for which $C = 3.51$ and $q = 52$ cu ft per sec per lin ft of spillway crest. The percentage increase in C then becomes the percentage increase in Q due to subatmospheric pressures at the interface. Table 8 shows the calculations, which are explained by the notes. Comparing the quantities in Cols. 4 and 11, they are seen to check fairly well with the limit of the curve in Fig. 10.

TABLE 8.—Increase in Discharge from Reduced Pressures Beneath the Nappe

Explanation: Col. 3, from Rouse-Reid experiments; Col. 4, same as increase of C (in Col. 3) over $C_d = 3.51$; Col. 5, calculated from $q = C\,(H_o)^{3/2}$; Col. 7, $H' = \left(\frac{q}{3.51}\right)^{2/3}$ (this would be the head if there were no subatmospheric pressure at the interface); Col. 9, read from curve of Fig. 10 with $\frac{H_o}{H'}$ as the argument; Col. 10 = Col. 9 × Col. 1; and Col. 11, read from curve of Fig. 10 with $\frac{p}{H_o}$ as the argument.

Head H_o	Ratio $\frac{H_o}{H_d}$	C	Increase in Q (%)	q	$\frac{q}{3.51}$	H'	$\frac{H_o}{H'}$	$\frac{p}{H_o}$	Feet of water p	Increase in Q_2 (%)
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
6	1.00	3.51	0	52	14.8	6.00	1.000	0	0	0
8	1.33	3.65	4	83	23.6	8.23	0.971	0.20	1.6	4.7
10	1.67	3.79	8	120	34.2	10.5	0.953	0.35	3.5	7.8
12	2.00	3.91	11	162	46.1	12.9	0.931	0.57	6.8	12.0
14	2.33	4.02	14	211	60.0	15.3	0.915	0.75	10.5	14.8
16	2.67	4.12	17	264	75.1	17.8	0.904	0.96	15.4	....
18	3.00	4.19	19	320	91.2	20.3	0.887	1.05	18.9	....
20	3.33	4.26	21	381	108.5	22.8	0.878	1.08	21.6	....
22	3.67	4.30	23	444	126.5	25.2	0.873	1.10	23.2	....

It is obvious from the foregoing that the savings in cost are important enough to warrant further development of the thesis that spillway discharges can be increased materially and safely by designing for subatmospheric pressures beneath the nappe.

This would be particularly useful for flood-control dams. The design head would produce no subatmospheric pressures; and hence all ordinary floods would

[11] "Model Research on Spillway Crests," by Hunter Rouse and Lincoln Reid, *Civil Engineering*, January, 1935, p. 10.

[12] "Fluid Mechanics for Hydraulic Engineers," by Hunter Rouse, McGraw-Hill Book Co., Inc., New York, N. Y., 1st Ed., p. 316.

[13] "Model Research on Spillway Crests," by Hunter Rouse and Lincoln Reid, *Civil Engineering*, January, 1935, p. 11, Fig. 2.

be passed with atmospheric pressure beneath the nappe. For the 1-in-100-yr or 1-in-1,000-yr flood, subatmospheric pressures of as much as $p = 30$ ft of water (which would increase discharges about 25%) could be provided for. This would permit either (1) reducing the spillway length to 80% of what would otherwise be required for the same head; (2) lowering the height of dam by 16% of the maximum overflow head and thereby reducing flood damages upstream; or (3) increasing the height of spillway crest for greater storage and power head if the pool level be fixed at some definite upper limit.

Little or no damage to the concrete, by pitting, could result from such intermittent use even if the pressure at the interface should fall to that of the water vapor and cavitation should result. If pitting should occur during a protracted flood, repairs could readily be made before the next flood would occur.

It would seem, therefore, that those agencies concerned with the design and construction of flood-control spillways would see that the experiments described by the author were extended to include a further study of spillways without crest gates. It would be a decided advantage, in such studies, if the models could be enclosed and the atmospheric pressure within adequately controlled.

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DISCUSSIONS

STRAINS, STRESSES, AND SHEAR IN ENGINEERING PROBLEMS

Discussion

BY MESSRS. ROBERTO CONTINI, I. NELIDOV, BERNARD L. WEINER, RICHARD W. ALBRECHT, AND DUFF A. ABRAMS

ROBERTO CONTINI,[14] JUN. AM. SOC. C. E.[14a]—Except for conclusions (*a*) and (*b*), the "Conclusions" of this paper are altogether correct. However, they are among the most elementary results of the Theory of Elasticity and have been known for decades. Moreover, the general approach is not correct.

For example, the definition of stress as given by the author does not have any physical meaning. In fact the stress, *f*, defined as (see heading, "Stress and Intensity of Force") "* * * the magnitude of internal force normal to unit area which is accompanied by, and proportional to, strain in the same direction * * *," is merely the strain in any direction multiplied by Young's modulus E. It is a quantity proportional to strain by definition and not because of any physical fact and, except for very particular cases, does not represent any actual force per unit area existing in the material.

The author states that it is common to conceive, and sometimes to define, stress as the normal component of the force known to be transferred across a unit area of cross section of a material. In fact, this is the definition of normal stress as used in the Theory of Elasticity and seems to be the only possible definition of normal stress.

A misinterpretation of Hooke's law that "strain is proportional to stress" seems to have occurred in this paper. It must be made clear that stress and strain are two entirely distinct physical concepts, and that each one is defined independently. For elastic materials they are related by Hooke's law, which, correctly quoted, states that strain components are linear functions of stress components. This does not mean at all that there is a constant of proportionality between stress and strain in any direction. The mathematical de-

NOTE.—This paper by Silas H. Woodard, M. Am. Soc. C. E., was published in February, 1943, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: April, 1943, by Messrs. C. M. Goodrich, Maurice P. van Buren, Joseph A. Wise, and David B. Hall.

[14] Eng. Asst., VegaAircraft Corp., Burbank, Calif.

[14a] Received by the Secretary April 16, 1943.

velopment of Hooke's law leads to the concepts of Young's modulus and Poisson's ratio, which are taken for granted in this paper.

When the author states (see heading, "Fundamental Problems of Stress in Terms of Strain"): "* * * If stress is proportional to strain * * *," he seems to establish some kind of Hooke's law for the material that he studies. However, this is only apparent because his "stress" is proportional to strain by definition, and a law between two interdependent quantities cannot have any physical meaning.

Using the notation and definitions of the author, it easily can be seen that the statement (see heading, "Stress and Intensity of Force") that "* * * f may be greater or less than p by a percentage not greater than Poisson's ratio, * * *" is not correct. In the case of Fig. 4, for $\alpha = 0$, $p_a = 0$ from Eq. 11*a*, and $f_a = -\mu p$ from Eq. 8*b*. The difference $p_a - f_a = \mu p$, expressed as a percentage of f_a, is 100, and as a percentage of p, infinity.

Referring to Fig. 3, it is very possible that some materials tested in compression fail when the transverse strain is equal to the strain in the direction of the load at failure for a tension test. This amounts to stating that in such cases strain, and not stress, determines the failure of the material. However, the material does not fail in tension, because there is no tensile force transmitted through planes parallel to the direction of compression. Perhaps the author should refer to failure by elongation.

Under the heading, "Shear and Stress," the author states that "* * * (*a*) in materials strained by exterior forces including shearing forces, only two kinds of stresses are set up—compressive stresses and tensile stresses—and (*b*) shear is accompanied by and resisted by internal forces of tension and compression." Under the heading, "Shear and Stress," the well-known fact is shown that (in two-dimensional stress distributions) there exist at any point two planes, perpendicular to each other, where no shear stresses exist and where normal stresses are maximum and minimum. From this it cannot be concluded, as the author does, that external forces are resisted within the material only along the two aforementioned planes (principal planes of stress). The principal planes are only two of an infinity of planes passing through a point. Stresses, shear stresses as well as normal ones, act along each of the infinite planes, and the material resists along each of the infinite planes. The principal planes are particularly interesting, but the planes of maximum shear (at 45° with the principal planes) are no less important. Shear stresses are neither "imaginary" nor "inexplicable" but are just as real as the principal stresses, and there is no proof, in this paper or elsewhere, that the material must fail along one of the principal planes. It may or it may not. It is perfectly conceivable, and verified by tension tests in steel, that materials may fail along the planes of maximum shear. Therefore conclusion (*b*), which should embody the main result of this paper, is incorrect.

Conclusion (*a*) is correct for stresses as defined by the author. However, since such stresses actually represent only strains, conclusion (*a*) would be more correctly stated if "strains" were read instead of "stresses." It has been proved in fact that stresses (as defined by the Theory of Elasticity) do

not depend on Poisson's ratio, in two-dimensional stress distributions, except in very particular cases.

In conclusion, not only does this paper present no new results, but it is extremely confused, and at odds with the fundamentals of the Theory of Elasticity.

I. NELIDOV,[15] M. AM. SOC. C. E.[15a]—The essential claims raised in this paper may be summarized briefly as follows:

1. Poisson's ratio should be considered in computations;
2. Shearing stress and strength are misnomers; and
3. Failure occurs when material is overstressed in tension.

In reading this paper, it is impossible to judge from its title or content whether the problem treated by the author refers to all materials or just to a particular one. It follows from the text, however, that, since the theory deals with the compression force acting on a prism, one may judge that the paper is intended for concrete. Some of the author's theses and conclusions seem to refer to concrete only, or, more generally, to brittle materials. However, since the author does not limit the scope of the paper, the result is confusing.

Referring to claim 1, one may regret that Poisson's ratio sometimes is unduly neglected in computations, and it is likewise without dispute that some engineers even raise their eyebrows at a mention of the so-called "true stresses." The term "true stress" is used herein according to the definition[16] of a stress formulated by the late Mansfield Merriman, M. Am. Soc. C. E., including the effect of Poisson's ratio, which the author calls simply "stress" and in contradistinction to Mr. Merriman's "apparent stress," which the author calls "force intensity." The terms chosen by Mr. Merriman are preferred by the writer because the term ("force intensity") used by the author is a somewhat more complicated one, whereas the term "stress" is so deeply rooted in terminology that to call it "apparent stress" or "true stress" will not be difficult. The stress is "apparent" because it results only from division of a force by an area without considering other factors. The "true stress" considers the effect of actual deformations.

However, a term is only a matter of choice of words. The principal thing should be the cognizance of the facts. From this point, the author's terminology will be used.

The importance of considering Poisson's ratio has been stated by the writer elsewhere,[17] at which time the "stress" in tension in a high buttress in the example chosen was found to be as high as 120 lb per sq in. in tension. The significance of neglecting this effect cannot be underestimated.

Eq. 10 and the effect of the Poisson's ratio,[18] as shown in Fig. 10, can be obtained directly from Mohr's circle.

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[15a] Received by the Secretary April 19, 1943.

[16] "Mechanics of Materials," by Mansfield Merriman, 11th Ed., 1914, John Wiley & Sons, Inc., New York, N. Y., p. 359.

[17] *Transactions*, Am. Soc. C. E., Vol. 106 (1941), p. 1326.

[18] "Mohr's Circle with Poisson's Ratio," by I. Nelidov, *Western Construction News and Highways Builder*, November 25, 1932, p. 666.

Thus, $f_n = f_{\max} - 2\, s_{\max} \cos^2 \alpha$. From the problem by the author: $f_{\max} = p$; and $f_{\min} = -\mu\, p$. From Mohr's circle: $s_{\max} = \frac{f_{\max} - f_{\min}}{2} = \frac{1 + \mu}{2}\, p$, or

$$f_n = p\,[1 - (1 + \mu) \cos^2 \alpha] \qquad (27)$$

It is evident that Eq. 27 is applicable only to materials that have a limit of proportionality of stress and strain and not to materials that have a modulus of elasticity which is a curved line from the beginning of loading. In this connection one wonders what the author has meant by his statement following Eq. 10 that "* * * Eq. 10 is applicable for all materials of construction up to the strains at which rupture occurs."

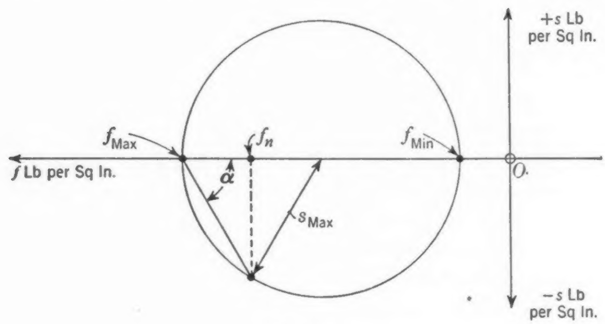

FIG. 10

In regard to claim 2, stated at the beginning of this discussion, the writer wishes to declare that, from a theoretical and conventional standpoint, one cannot dispose so easily of the terms "shearing stress" and "shearing strength," because tangential forces and resistance will always exist, disregarding the designer's wishes to be rid of them. It is true that in actual practice the true shearing force or stress is never approached because a case of true shear will call for two forces acting one against the other and nearly collinear. In actual problems shearing forces will have a lever arm always, thus forming a couple; or they will have a bending moment, with resulting tensile stress at right angles to the direction of shear. The reason that the term "shearing strength" exists is explained in the same way as the reason for the existence of the term "compressive strength." Neither of these terms has any real meaning, because both are obtained from the simplest possible way of loading a specimen, one in shear, another in direct compression. If one is desirous of finding the correct factor of safety for a material, one will have to test a specimen under the same conditions of lateral support in regard to such tension, compression, or shear as will exist in the full-sized structure and thus will determine its strength. Because of the complications involved in this kind of test, it is very seldom made; instead, judgment is supported by simple compressive, tensile, or other tests.

That definite shearing stresses exist if Poisson's ratio is considered can be proved from Fig. 3, concerning which the author quotes the well-known fact

that, "if friction [at the ends of a prism under compression] be eliminated by effective lubrication, the diameter of the entire cylinder will expand and ultimately the material will fail by cracking parallel with the direction of compression * * *." What actually happens in this case is that the tensile strength of the material is greater than the shearing strength, which is zero at both ends. The specimen fails in shear at both ends due to lateral expansion long before it fails "in tension." However, the shearing failure is not conspicuous and is not noted, but the tensile failure is manifest and thus is wrongly named. If the ends are cast monolithically with the end supports, or even just restrained, as in ordinary compression tests, then tensile strength will become smaller than the shearing strength and failure will occur in tension on inclined planes, which again is erroneously called "shear failure." Shearing stress or force intensity is inseparable from tensile or compressive stress because it simply manifests angular distortion just as tensile or compressive stress manifests axial deformation.

Therefore, the writer disagrees with Mr. Woodard in regard to claim 2 and thinks that, conventionally, theoretically, and actually, the author's shearing "force intensity" and "stress" do exist and are not misnomers. In regard to claim 3 the following may be said:

There is danger in making quick conclusions regarding the stress condition and possible cause of failure based on the assumption of a uniform distribution of a stress. To illustrate, consider the prism used by the author of finite dimensions and under compressive load. His reasoning applies only to a theoretical case of a unit area wherein a force p is acting on an elementary cube under consideration. In an actual prism, a total force P is not even equally distributed on the bearing ends, and much less within the prism. Therefore, the generalization by the author derived from his elementary formula to explain the cause of failure of a finite body may be considered as a first step only. A further step would be to determine a true distribution of stresses and to observe the points where various stresses are excessive. Failure may begin at any of these points, and it will always be progressive. Usually, since the speed of progress of a failure is great, it is considered that the entire section failed at once, and some "average" stress is computed which caused this failure. This may be practical, but it is incorrect and the discrepancy is demonstrated best by failures of machine parts. These fail in many instances when the "average" stresses appear to be perfectly safe.

The best illustration of the writer's attitude on the causes of failure may be given in the following quotation from S. Timoshenko[19] (to which the writer has nothing to add):

> "In the case of brittle materials such as cast iron, fracture occurs without appreciable plastic deformation and on a cross-section perpendicular to the direction of tension. This is a separation failure. A specimen of a ductile material such as mild steel undergoes considerable plastic deformation and reductions in cross-sectional area due to sliding along planes inclined 45° to the axis of the specimen before fracture occurs. This is sliding failure. * * * If the resistance to sliding is greater than the re-

[19] "Strength of Materials," by S. Timoshenko, Pt. II, 1930 Ed., p. 671.

sistance to separation, we have a brittle material and fracture will occur as a result of overcoming cohesive forces without appreciable deformation. If the resistance to separation is larger than resistance to sliding, we have a ductile material. * * * The relation between the resistance to separation and the resistance to sliding does not remain constant for the same material. It depends very much upon the velocity of deformation and upon the temperature at which a test is made. There are evidences that the resistance to sliding increases as the velocity of deformation increases and as the temperature is lowered. At the same time the resistance to separation is not affected to the same degree by these two factors. This would explain why a bar of a metal such as zinc can be bent like a ductile material under slow loading while the same bar fractures without plastic deformation if the loading is applied suddenly. * * * The type of fracture depends also on the manner of testing. If the loading is of such a nature that fracture due to separation is prevented, a considerable plastic deformation may be obtained in a material usually considered brittle. * * * Likewise, a ductile material may have a fracture of the brittle type if the form of the specimen or the type of stress distribution is such that plastic deformations, due to sliding, are prevented."

Based on his own previous statements and the foregoing quotation, the writer does not agree with Mr. Woodard's conclusions condensed in claim 3.

To summarize, the writer is of the opinion that the author has raised an important issue in the use of Poisson's ratio, but his conclusions about shear stresses and causes of failure cannot be verified.

BERNARD L. WEINER,[20] M. AM. SOC. C. E.[20a]—The laws of static equilibrium hardly can be questioned, at this late date. Although the mathematical theory of elasticity is not so well known, it is too well recognized to be questioned lightly. Finally, the ellipse of stress has been accepted for so long that it can be questioned only by offering the most exhaustive proof. The author's work is in direct contradiction of all three and, without exhaustive proof, is suspect. There can be little doubt that his work is in error.

Mr. Woodard proceeds to find the strains normal to a plane at an angle to an axial force acting on a rectangle and then argues that since stress is proportional to strain in the same direction, the stress is E times the strain. Let it be said once, and for all, that stress and strain in the same direction are not directly proportional to each other—except in one special case. The general case, which can be found in any standard text on the mathematical theory of elasticity and in texts on advanced mechanics of materials, is:

$$\left.\begin{aligned} \delta_1 &= \frac{p_a}{E} - \frac{\mu}{E}(p_b + p_c) \\ \delta_2 &= \frac{p_b}{E} - \frac{\mu}{E}(p_a + p_c) \\ \delta_3 &= \frac{p_c}{E} - \frac{\mu}{E}(p_b + p_c) \end{aligned}\right\} \qquad (28)$$

[20] Designer, Parsons, Klapp, Brinckerhoff & Douglas, New York, N. Y.

[20a] Received by the Secretary April 26, 1943.

and

$$\left.\begin{aligned} \phi_1 &= \frac{s_a}{G} \\ \phi_2 &= \frac{s_b}{G} \\ \phi_3 &= \frac{s_c}{G} \end{aligned}\right\} \qquad (29)$$

in which s = unit shear; G = the shear modulus of elasticity; ϕ = angle of shear strain; and δ = unit elongation.

If only one force and its reaction are acting, it is true that stress and strain parallel to the force are proportional; but, even in this case, strain perpendicular to the force exists without stress. If the stresses are unknown but the strains are given, the former can be found by solving the general equations of the stress-strain relationship. If the strains in this simple case were substituted in these equations, p_a would be the only one having a finite value equal to p_a, of course, and p_b and p_c would equal zero. This is a matter of simple algebra—and no mysterious theories are involved.

What the author is really doing is to revive the old exploded theory of "virtual stress" as against "real stress." This conception has been renamed "intensity of force" and "intensity of stress" but is otherwise the same idea derived from the fallacy that stress and strain in the same direction are directly proportional. In his "Mechanics of Materials,"[21] Mr. Merriman made the same error probably more than half a century ago, and others have followed in his footsteps from time to time. Many "odd" results can be obtained, of course, by the use of Eq. 8*b*, but only one is necessary to show the error. If, in this equation, angle α is set equal to zero, $f_n = -(1 + \mu)\,p$; but since the plane that makes a zero angle with the only applied force is parallel to, and may be, an external surface, it cannot have any stress on it. Other results, although not so obvious, are equally incorrect.

Since the equation $\frac{\delta_n}{\delta} = \frac{f_n}{p}$ is incorrect, the results derived for the intensity of stress have no meaning. The author should have taken warning that something was radically wrong with his theory. No matter what else is involved, the fundamental laws of static equilibrium still must be satisfied under any and all conditions and a summation of the stresses on a plane must equal zero (taking external forces, if any, into account). As a matter of fact, for the simple case treated in Fig. 4, the intensity of force is also the true and synonymous intensity of stress and the author's version is merely a fallacy that does not exist.

There is no need to go into this part of the paper any further since the true equations have been developed in standard texts on the mathematical theory of elasticity and in texts on advanced mechanics of materials. It may be well to point out, however, that, for the general case, the stresses can be found only by solving partial differential equations. The fact that this is usually the

[21] "Mechanics of Materials," by Mansfield Merriman, 11th Ed., John Wiley & Sons, Inc., New York, N. Y., 1914.

case is rather unfortunate because, in the present state of knowledge of this type of equations, most of them cannot be solved directly. John Prescott,[22] in his "Applied Elasticity," makes this point very clear.

Most of the author's equations are merely variations of the ellipse of stress derived by very laborious reasoning; and, when he does not tie them up with the fallacy of virtual stress (intensity of stress), they are correct. The derivations are arrived at much more simply in standard texts. However that may be, although the author's equations—without the virtual stress fallacy—are correct, his interpretations of the meaning of the equations are incorrect. Shear does exist and the reasoning which leads to the ellipse of stress simply states that a force which produces tension on one plane, compression on another, may, and usually does, produce shear on the same or other planes. Instead of denying the existence of shear, the real interpretation is that the material must be strong enough to resist the stresses in all directions, whatever they may be —tension, compression, or shear, or a combination of direct stress and shear. It might just as well be argued that compression does not exist in certain members of a truss, because the forces really are resisted, of course, by tension in other members. In Fig. 4, if line A-A were a cleavage plane, the upper part would certainly slide over the lower and it would be a sliding or shear failure.

According to the author's discussion in connection with Fig. 3, the block under compression would fail in tension—if the bearings were lubricated. Quite the contrary—as is well known in statically indeterminate stress analysis, only when "normal" deformation is prevented, are stresses produced. Deformation itself, unrestrained, does not produce any stress. The old illustration of the unrestrained bar which is heated and of the same bar which is confined between rigid abutments and also heated is sufficient to prove the fallacy of the author's contention. In Fig. 3, the very fact that the expansion of the diameter as measured by Poisson's ratio is prevented by the friction between the bearings and the block produces the stress at right angles to the force. The stress induced, with some modification that is unnecessary to go into for the present purpose, is the same as if the unstressed block were deformed by a system of radial, transverse forces by an amount equal to the Poisson expansion. Here again, of course, the fallacy lies in the incorrect assumption that stress and strain in the same direction are always directly proportional to each other.

Although the discussion in connection with Fig. 3 is in error, it does call attention to a problem which needs to be solved. The lateral stresses produced by the restraint will produce other stresses, in turn, and the solution, the writer ventures to guess, will not be either simple or easy. Further study is necessary to solve this problem, which may yield valuable results.

Since most of the paper is merely a laborious variation of the ellipse of stress erroneously combined with Poisson's ratio, most of the conclusions are invalid. The results derived in Table 1 are invalid also, but, by accident, they are probably not very seriously in error because of the fact that the applied tensions and shears are of the same order of magnitude. This fact tends to cancel the error. Hence, whatever meaning the data may have, they are very likely approximately correct.

[22] "Applied Elasticity," by John Prescott, Longmans, Green, London, 1924.

This paper is merely a compilation of old and new fallacies; and it controverts established fact.

RICHARD W. ALBRECHT,[23] JUN. AM. SOC. C. E.[23a]—A refinement of the Maximum Strain Theory of failure is presented in this paper. The usual method of treating this theory is to find the unit strain due to the maximum principal stress, modify this value by the strain caused by the stress at right angles to the maximum principal stress, and call the resulting value the maximum strain. The paper presents the method of first finding the unit strain in terms of the normal stress and the stress at right angles to this stress, and then maximizing the result to obtain the maximum unit strain. Although the latter method is theoretically more accurate, the difference between the values obtained in this manner and the values obtained in the usual manner is negligible and does not justify the use of the more cumbersome equations evolved. The evaluation of Poisson's ratio and the modulus of elasticity are more approximate than the assumption made in the usual derivation of the Maximum Strain Theory, which makes the more accurate assumption useless except for a mathematical derivation of the theory. A comparison of the values of the angle β and the maximum tensile and compressive unit stresses as obtained by the use of Eqs. 21 and 15 (which differ from the tabulated values in the paper) and as obtained by the usual Maximum Strain Theory, is given in Table 6. All

TABLE 6.—COMPARISON OF MAXIMUM AND MINIMUM STRESSES, IN POUNDS PER SQUARE INCH, COMPUTED BY EQS. 21 AND 15 AND BY THE USUAL MAXIMUM STRAIN THEORY

(For Values of Applied Shear and Tension, See Table 1)

Test No.	Eqs. 21 and 15			Usual Maximum Strain Theory		
	β	Computed Stress		β	Computed Stress	
		Tensile	Compressive		Tensile	Compressive
3SA3	63°−39′	22,480	11,950	63°−40′	22,470	11,970
3SA24	62°−56′	21,910	11,930	62°−57′	21,890	11,900
3SA15	60°−24′	21,560	12,715	60°−26′	21,540	12,690
3SA18	60°−59′	21,950	12,720	60°−59′	21,930	12,700
3SA21	61°−27′	22,280	12,720	61°−27′	22,290	12,720
3SA4	57°−14′	22,720	14,880	57°−15′	22,700	14,850
3SA20	59°−22′	24,220	14,780	59°−23′	24,220	14,760
3SA10	56°− 0′	21,700	14,800	56°− 0′	21,730	14,820
3SA17	55°−41′	21,520	14,850	55°−41′	21,510	14,855
3SA7	45°− 0′	17,420	17,420	45°− 0′	17,420	17,420

differences are within the limits ordinarily to be expected when a slide rule is used.

In Conclusion (*b*) the author states:

"Shear intensity is not a stress but is always accompanied and resisted by diagonal tensile or compressive stresses or both."

[23] Instr., School of Technology, College of City of New York, New York, N. Y.

[23a] Received by the Secretary May 4, 1943.

This statement is correct only when two specific planes at right angles to each other are considered. There are an infinite number of other planes on which a tendency to slide exists. If the material is unable to furnish the necessary resistance to sliding but is at the same time able to furnish the necessary resistance to compression or tension, a sliding failure will occur along the plane on which the maximum shearing stress exists. Such failure occurs in a brittle material subjected to compression alone. Theodor von Kármán, M. Am. Soc. C. E., and Robert Böker conducted extensive studies of the theories of failure of a brittle material. Their results indicate that the failure of a material may be controlled by a dual law, a tensile or splitting failure occurring with certain stress combinations and a shearing or sliding failure occurring with other combinations.[24]

The author (see heading "Stress and Intensity of Force") "enlists with the partisans of the Maximum Stress and Maximum Strain Theories of failure as opposed to the Maximum Shear Theory, the Maximum Distortion Energy Theory, and the Maximum Strain Energy Theory." Can it be said that these theories are opposed to each other? Rather, should it not be said that these theories are various attempts to explain on a rational basis the failure of engineering materials? As no one of these theories deals with the atomic or molecular structure of the material, each theory makes certain basic assumptions which may or may not be exactly correct. The best test of any theory is its agreement with observed data. The five theories of failure under discussion are as follows:[25]

Maximum Stress Theory.—

$$\sigma_w = \frac{\sigma_x + \sigma_y}{2} \pm \sqrt{\left(\frac{\sigma_x - \sigma_y}{2}\right)^2 + (\tau_{xy})^2} \ldots\ldots\ldots\ldots (30a)$$

Maximum Shear Theory.—

$$\sigma_w = 2\sqrt{\left(\frac{\sigma_x - \sigma_y}{2}\right)^2 + (\tau_{xy})^2} \ldots\ldots\ldots\ldots (30b)$$

Maximum Strain Theory.—

$$\sigma_w = (1 - \mu)\left(\frac{\sigma_x + \sigma_y}{2}\right) + (1 + \mu)\sqrt{\left(\frac{\sigma_x - \sigma_y}{2}\right)^2 + (\tau_{xy})^2} \ldots (30c)$$

Strain Energy Theory.—

$$\sigma_w = \sqrt{(\sigma_x)^2 - 2\,\mu\,\sigma_x\,\sigma_y + (\sigma_y)^2 + 2\,(1 + \mu)\,(\tau_{xy})^2} \ldots\ldots (30d)$$

Distortion Energy Theory.—

$$\sigma_w = \sqrt{(\sigma_x)^2 - \sigma_x\,\sigma_y + (\sigma_y)^2 + 3\,(\tau_{xy})^2} \ldots\ldots\ldots\ldots (30e)$$

[24] "Die Mechanik der bleibenden Formänderung in kristallinisch aufgebauten Körpern," by Robert Böker, *Mitteilungen über Forschungsarbeiten auf dem Gebiete des Ingenieurwesens*, Vol. 176 and 176, 1915 (for reference to this subject, see "A Study of the Failure of Concrete under Combined Compressive Stresses," by F. E. Richart, A. Brandtzaeg, and R. L. Brown, *Bulletin No. 185*, Univ. of Illinois, Urbana).

[25] "Mechanical Properties of Materials and Design," by Joseph Marin, McGraw-Hill Book Co., Inc., 1942.

For the illustration used in the paper, in which $\sigma_y = 0$ and $\mu = 0.33$, Eqs. 30 reduce to the forms:

Maximum Stress Theory.—

$$\sigma_w = \frac{\sigma_x}{2} \pm \sqrt{\left(\frac{\sigma_x}{2}\right)^2 + (\tau_{xy})^2} \qquad \text{(31a)}$$

Maximum Shear Theory.—

$$\sigma_w = 2\sqrt{\left(\frac{\sigma_x}{2}\right)^2 + (\tau_{xy})^2} \qquad \text{(31b)}$$

Maximum Strain Theory.—

$$\sigma_w = 0.335\ \sigma_x + 1.33\sqrt{\left(\frac{\sigma_x}{2}\right)^2 + (\tau_{xy})^2} \qquad \text{(31c)}$$

Strain Energy Theory.—

$$\sigma_w = \sqrt{(\sigma_x)^2 + 2.66\ (\tau_{xy})^2} \qquad \text{(31d)}$$

Distortion Energy Theory.—

$$\sigma_w = \sqrt{(\sigma_x)^2 + 3\ (\tau_{xy})^2} \qquad \text{(31e)}$$

A comparison of the values of the failure stresses, σ_w, as found by the various theories, and the ratio of this stress to the proof stress, σ_p, is given in Table 7.

TABLE 7.—COMPARISON OF FAILURE STRESSES COMPUTED BY VARIOUS FAILURE THEORIES

(For Values of Applied Shear and Tension, See Table 1)

Test No.	$\frac{\sigma_x}{\tau_{xy}}$	FAILURE STRESS, σ_w (LB PER SQ IN.)					$\frac{\text{FAILURE STRESS}}{\text{PROOF STRESS}} = \frac{\sigma_w}{\sigma_p}$				
		Stress theory	Shear theory	Strain theory	Strain energy theory	Distortion energy theory	Stress theory	Shear theory	Strain theory	Strain energy theory	Distortion energy theory
3SA3	1.135	20,800	25,900	22,470	22,800	23,800	0.889	1.107	0.960	0.984	1.018
3SA24	1.445	20,150	25,400	21,890	22,300	23,250	0.861	1.085	0.935	0.952	0.996
3SA15	1.192	19,470	25,740	21,540	22,350	23,250	0.832	1.100	0.920	0.955	0.996
3SA18	1.249	19,920	26,040	21,930	22,700	23,600	0.839	1.113	0.936	0.970	1.009
3SA21	1.293	20,320	26,340	22,290	23,000	23,950	0.867	1.125	0.952	0.982	1.023
3SA4	0.920	19,960	28,220	22,700	24,200	25,400	0.853	1.205	0.970	1.033	1.085
3SA20	1.098	21,700	29,300	24,220	25,250	26,350	0.926	1.252	1.035	1.079	1.125
3SA10	0.820	18,890	27,480	21,730	23,200	24,400	0.806	1.173	0.928	0.992	1.042
3SA17	0.782	18,655	27,360	21,510	23,000	24,200	0.797	1.168	0.918	0.983	1.034
3SA7	0	13,100	26,200	17,420	21,400	22,700	0.560	1.120	0.745	0.915	0.970

In Fig. 11, this ratio is plotted as ordinate against the ratio of applied tension to applied shear as abscissa. An attempt has been made to plot probable curves for the comparisons of the various theories of failure they may give, although data are lacking for enough points to give conclusive curves. These curves show the most conservative theory for the test conditions to be the Maximum Shear Theory, and the least conservative to be the Maximum Stress Theory.

The theory that gives results closest to the observed data is the Distortion Energy Theory.

The application of these theories to the data given in the paper is of doubtful value as they purport to explain only the failure of elastic action, whereas the

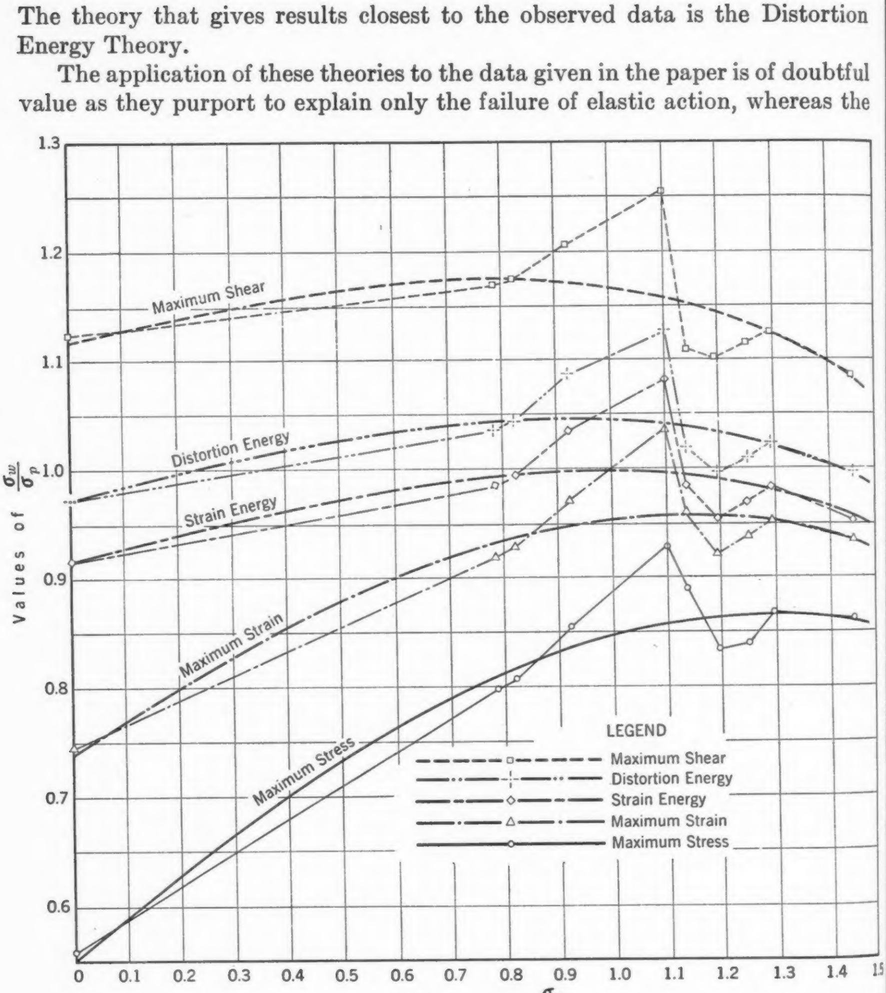

FIG. 11.—RATIO OF FAILURE STRESS TO PROOF STRESS BY VARIOUS FAILURE THEORIES

stresses given in the paper are apparently for complete failure, being considerably higher than the 17,500 lb per sq in. yield point of the material.

DUFF A. ABRAMS,[26] M. AM. SOC. C. E.[26a]—It should not be forgotten that strength of materials is a "land of make believe." Approaching the subject from the mathematical viewpoint, one must assume a material with idealized properties and load conditions that have little relation to anything in nature. Assumptions generally include isotropic material, constant elastic properties, and simple axial stress within the elastic range. Approaching the subject from

[26] Cons. Engr., New York, N. Y.

[26a] Received by the Secretary May 6, 1943.

the physical viewpoint, one soon encounters distinct limitations to experimental methods that lead to many contradictions. Probably no one knows the actual strength of any material. Determinations that have been made are based on arbitrary, and frequently unskillful test methods, all of which conceal important assumptions or fallacies that have been generally overlooked or ignored.

Most tests have been made in tension; yet a cursory consideration will show that pure and uniform tension is never developed. If such questions as boundary conditions, surface imperfections, localized stresses, sustained, reversed or repeated loads are considered, all the usual concepts of strength of materials must be modified or discarded.

As early as 1858 A. Wöhler[27] made fatigue tests on steel bars that failed at loads much lower than their strength as determined by once-applied loads. Investigations of fatigue of modern steels[28,29] showed that with 100,000,000 reversals of stress failure occurred at 40% to 70% of the usual tensile strength.

In 1907 J. L. Van Ornum,[30] M. Am. Soc. C. E., showed that both concrete prisms in compression and reinforced concrete beams in flexure failed under 10,000 to 32,000 repetitions at about 50% of the load they would withstand if loaded to their ultimate strength at once.

In 1920, A. A. Griffith[31] showed that the tensile strength of a certain glass varied enormously with the diameter of the specimen. For diameter 0.04 in. the strength was 24,900 lb per sq in.; and, for a diameter of 0.00013 in., it was 491,000 lb per sq in. Griffith estimated that if this glass were drawn to molecular diameter, it would have a tensile strength of 1,600,000 lb per sq in. Tests of metal wires have shown somewhat similar results.

The foregoing examples emphasize some of the properties of materials that are concealed by the "factor of ignorance," usually referred to as a "factor of safety."

Effect of Elastic Beds on Compressive Strength.—During the past century, considerable literature has accumulated on the effect of the end condition of test specimens on the compressive strength of cast iron, stone, concrete, brick, and similar materials. This problem is not so simple as indicated by Mr. Woodard's analysis; the evidence indicates that in addition to the end condition, the method of failure of short compression specimens is influenced by: (*a*) The rigidity of the material under test; (*b*) the size of specimen; and many other factors.

In 1773, C. A. Coulomb[32] concluded that a material loaded in compression will fail along a series of planes at 45° with the horizontal; in the case of a cube, six equal tetrahedra are formed, each having as its base one face of the original cube. Observation shows that, in general, this so-called pyramidal

[27] "Zeitschrift für Bauwesen," by A. Wöhler, Vol. 8–20, 1860–1870 (summary in *Engineering*, Vol. 4, 1867, and Vol. 11, 1871).

[28] "The Engineer," by H. J. Gough, London, August 12, 1921; also "The Fatigue of Metals," by H. J. Gough, Scott, Greenwood & Son, London, 1924.

[29] "An Investigation of the Fatigue of Metals," by H. F. Moore and J. B. Kommers, *Bulletin No. 124*, Univ. of Illinois Eng. Experiment Station, Urbana 1921.

[30] "The Fatigue of Concrete," by J. L. Van Ornum, *Transactions*, Am. Soc. C. E., Vol. LVIII (June, 1907), p. 294.

[31] "Phenomena of Rupture and Flow in Solids," by A. A. Griffith, *Philosophical Transactions*, Royal Soc. of London, Vol. 221, 1920, p. 181.

[32] *Mémoirs* of the Royal (French) Academy of Sciences, 1773, p. 343.

fracture occurs only in materials of comparatively low strengths, such as the weaker rocks, or mortars and concretes at early ages.

Mr. Woodard states with reference to this method of failure (see heading, "Fundamental Problems of Stress in Terms of Strain"):

> "* * * if friction be eliminated by effective lubrication, the diameter of the entire cylinder will expand and ultimately the material will fail by cracking parallel with the direction of compression, as indicated in Fig. 3."

In making early compression tests of building stones, it was customary to place thin sheets of lead under and over the test cube. The weakening effect of lead sheets was brought prominently to the attention of American engineers by the report of the Federal Commission of 1851 that was appointed to study marbles, etc., in connection with the extension of the U. S. Capitol; details are given in the architect's report that accompanied the message of President Millard Fillmore to the 32d Congress, 2d Session, in 1852. With reference to these tests it may be appropriate to quote from a paper dated 1873 by C. B. Richards:[33]

> "This Commission discovered the somewhat remarkable fact that the effect of pieces of sheet lead placed between the stone samples and the steel surfaces of the testing apparatus, was to occasion the failure of the specimen with about half the load it would sustain if pressed directly by the steel surfaces; that is, with the steel and stone surfaces in contact."

The findings of the Commission of 1851 on the use of lead sheets in testing building stone should be compared with the somewhat similar results in Mr. Woodard's compression tests of cement and mortar specimens with lubricated ends.

The effect on the strength of various end treatments of test specimens was brought out for stone cubes by Q. A. Gillmore,[34] the late W. C. Unwin,[35] Hon. M. Am. Soc. C. E., G. P. Merrill,[36] and by the tests made by the Chief of Engineers, U. S. Army[37]; and for concrete cylinders by D. V. Terrell, M. Am. Soc. C. E.,[38] and H. F. Gonnerman,[39] M. Am. Soc. C. E.

The effect of lubrication of the type used by Mr. Woodard on the strength and type of failure may be very different with different materials. Mr. Woodard's tests covered only the use of greased blotting paper. Sufficient data are available to throw much doubt on the author's conclusions when applied to the general case of short specimens in compression. Lubrication of the ends of specimens is not necessary to produce failure by "cracking parallel with the direction of compression." In 1859, W. Fairbairn and T. Tate[40]

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[33] "Experiments on the Resistance of Stones to Crushing," by C. B. Richards, *Transactions*, Am. Soc. C. E., Vol. II (1874), p. 192.

[34] "Notes on the Compressive Resistance of Freestone, Brick Piers, Hydraulic Cements, Mortar and Concrete," by Q. A. Gillmore, John Wiley & Sons, Inc., New York, N. Y., 1888.

[35] "The Testing of Materials of Construction," by W. C. Unwin, 1888, p. 416.

[36] "Stones for Building and Decoration," by G. P. Merrill, John Wiley & Sons, Inc., New York, N. Y., 3d Ed., 1910.

[37] "Building Stones," Rept. of Chf. of Engrs., U. S. Army, Appendix II, 1875.

[38] "Irregular Ends on Concrete Test Cylinders," by D. V. Terrell, *Engineering and Contracting*, August 1, 1923.

[39] "Effect of End Condition of Cylinder on Compressive Strength of Concrete," by H. F. Gonnerman, *Proceedings*, A.S.T.M., Vol. 24, Pt. II, 1924.

[40] "On the Resistance of Glass Globes and Cylinders to Collapse from Internal Pressure, and on the Tensile and Compressive Strength of Various Kinds of Glass," by W. Fairbairn and T. Tate, *Proceedings*, Royal Soc. of London, May 12, 1859.

reported compression tests of glass cubes that showed this type of failure. They stated:

> "The specimens were crushed almost to powder by the violence of the concussion, when they gave way; it, however, appeared that the fracture occurred in vertical planes, splitting up the specimen in all directions. Cracks were noticed to form some time before the specimen finally gave way; then these rapidly increased in number, splitting the glass into innumerable irregular prisms of the same height as the cube. Finally, these bent or broke, and the pressure, no longer bedded on a firm surface, destroyed the specimen."

From 1900 to 1912 there was a lively discussion of the effect of elastic substances interposed between the loading surfaces in compression in the *Proceedings* of the International Association for Testing Materials.[41,42,43]

Capping of Concrete Cylinders.—An elaborate investigation of methods of capping the tops of 6-in. by 12-in. concrete cylinders, conducted under the writer's direction in 1923, showed that the strength of concrete was reduced by practically all of the yielding materials used. The principal data of these tests are given in Table 8, rearranged from Mr. Gonnerman's report.[39] The standard method consisted of a neat cement cap about $\frac{1}{8}$ in. thick applied to the fresh concrete 2 to 6 hours after molding the cylinders, by means of a planed cast-iron cover plate.

The values at the tops of the columns of strength ratios in Table 8 are the compressive strengths for standard capping method; each of these was based on 49 to 58 tests. Some of the conclusions are:

(*a*) Cylinders capped just prior to test with gypsum or mixtures of cement and gypsum gave sensibly the same strength as the standard method using neat cement.

(*b*) In general, compressible materials caused a loss in strength; the greatest loss was in the strongest concrete.

(*c*) The loss in strength from the use of sheet lead was about the same for thicknesses varying from 1/16 in. to 3/16 in., and was sensibly the same as for blotting paper or cork. For the strongest concrete (4,500 lb per sq in. at 28 days) the strength ratios from these materials was about 0.75.

(*d*) The greatest reduction in strength was found with sheet rubber; for the 1/16-in. thickness the ratios for the strongest concrete were 0.47 to 0.65.

The strength ratios for blotting paper in these tests and for those of Mr. Woodard using grease and blotting paper are as follows (length of specimens, two diameters):

Tests by	Weakest specimens	Strongest specimens
Gonnerman	0.92	0.77
Woodard	0.72	0.66

[41] "Baumaterialeinkunde," 1900, by A. Föppl, p. 129, and by F. Kick, p. 177.

[42] "On the Resistance of Stone to Compression, with Elastic Substances Interposed between the Surfaces in Compression," by S. C. Pace, *Paper B. 10e*, Brussels Cong., International Assn. for Testing Materials, 1906.

[43] "On the Question of Friction between the Parallel End Surfaces of a Body and the Plates through which Pressure Is Applied," by H. I. Hannover, *Proceedings*, VIth Cong., International Assn. for Testing Materials, Paper XXVII$_7$, 1912.

It should be noted that in Mr. Gonnerman's tests only the tops of the cylinders were affected by the various capping materials and methods; if the bottoms had been treated in the same manner, the observed reduction in strength would probably have been nearly doubled.

TABLE 8.—COMPRESSION TESTS OF CONCRETE CYLINDERS, USING DIFFERENT CAPPING MATERIALS

Capping material	Time[a]	Thickness of caps (in.)	STRENGTH RATIOS, LB PER SQ IN.[b]		
			1,080	2,150	4,500
Concrete mix by volume[c]	....	....	1 : 7	1 : 5	1 : 3.5
Neat cement; standard cap	....	....	1.00	1.00	1.00
Top smoothed by trowel; no cap	....	....	0.96	0.94	0.80
Top smoothed by grinder; no cap	....	....	1.00	1.06	1.03
Gypsum	0.25	....	0.97	1.04	1.01
	0.50	....	0.95	0.99	1.02
	1.00	....	0.96	1.01	1.03
	3.00	....	0.96	0.95	1.00
	6.00	....	0.97	1.01	0.99
	24.00	....	0.95	1.01	1.01
Cement-gypsum (1 : 1)	0.50	....	1.02	1.04	0.97
	1.00	....	1.02	1.00	0.98
	3.00	....	1.03	1.00	0.97
	6.00	....	1.00	0.98	1.00
	24.00	....	1.01	1.02	0.98
Cement-gypsum (3 : 1)	6.00	....	1.02	1.02	0.99
Cement-gypsum (1 : 3)	6.00	....	1.01	0.99	1.02
Beaver board	....	3/16	0.98	1.01	0.91
White pine	....	1/8	0.92	0.99	0.87
Millboard	....	1/16	1.01	0.92	0.81
Leather	....	9/32	0.96	0.91	0.82
Blotting paper	....	3/32	0.92	0.97	0.77
Cork carpet	....	5/16	0.93	0.87	0.70
Sheet lead	....	1/16	0.95	0.88	0.74
	....	1/8	0.91	0.90	0.74
	....	3/16	0.89	0.88	0.77
Sheet rubber	....	1/16	0.81	0.71	0.47
	....	1/8	0.89	0.77	0.65
	....	1/4	0.93	0.80	0.63

[a] Time of capping, in hours before test. [b] Cylinders were made of a mixture of four brands of portland cement purchased in Chicago, Ill. Values given are the averages of ten tests made on different days. [c] Flow about 220.

Tensile Strength of Mortar Briquets.—Mr. Woodard states (see heading, "Fundamental Problems of Stress in Terms of Strain"):

> "* * * the known tensile strength, divided by the observed compressive stress at time of failure equals Poisson's ratio as nearly as it can be determined."

This conclusion was based on tensile strengths of cement and mortar briquets of the form that are standard for cement tests,[44] and on compression tests of cylinders and prisms. If the quotient of these strengths equals Poisson's ratio, it was probably accidental. Several different methods of determining the tensile strength of mortars and concretes have been used. Each method gives a different tensile strength; and the briquet is probably the most unsatisfactory form of all. Since mortars fail at an elongation of about 0.001, and the most-stressed section of a briquet is only a fraction of an inch long, it is obvious that there is little opportunity for the stress to become uniformly

[44] "Standard Methods of Sampling and Physical Testing of Portland Cement," A.S.T.M. Standards, Pt. II, 1942, p. 33.

distributed across the minimum section. The briquet represents a clear case of eccentric loading.

C. L. Durand-Claye[45] in 1895 gave an analytical method of calculating the variations of stress across the minimum section of a briquet. A. Föppl[7] in 1896 showed wide variations in the stresses in a briquet by measurements on an india rubber model. The late J. B. Johnson,[46] M. Am. Soc. C. E., in 1897 applied Durand-Claye's method of analysis to briquets of the form used in the United States. E. G. Coker,[47] in 1912 showed the wide range in these stresses by means of photoelastic observations on glass or celluloid models; and similar methods were used in 1939 by F. O. Anderegg, Royal Weller, and B. Fried.[48] These studies showed a high concentration of stresses at the edges of the waist of the U. S. Standard briquet, as follows:

Authority	Date	Ratio of maximum to average stress
Johnson	1897	1.54
Coker	1912	1.70
Anderegg	1939	1.78
Average		1.67

The failure of the briquet results from a tearing action rather than from a true tensile stress. As the cement becomes older and stiffer, the discrepancy between the indicated and the true strength becomes more and more pronounced. This accounts for phenomena that have been observed many times —namely, a rapid increase in tensile strength of cement mortars to 1 to 2 months followed by a gradual falling off, until the indicated briquet strength at 1 year was generally less than at 7 days, whereas the compressive strength of the same mortars and of concrete from the same cement continued to increase indefinitely, as long as the specimens did not dry out.

The briquet is a carry-over from the infancy of the art. It was adapted by John Grant[49] in London in 1859 from the practice of stone testing, and has undergone little change during the succeeding 84 years. In Germany the briquet was abandoned in favor of the cube for cement mortar tests a generation ago. The inadequacies of the briquet have been commented on by a number of American writers in addition to those already referred to.[50,51,52] Erroneous attempts to reconcile the composition of cement and the behavior of concrete with the falling-off in briquet strength are responsible for much of the confusion that has characterized the history of cement and concrete technology in the United States.

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[45] *Annales des Ponts et Chaussées*, Durand-Claye, June, 1895.

[7] "Vorlesungen über Technische Mechanik," by August Föppl, 9th Ed., III 68; see also "Zwang und Drang."

[46] "Materials of Construction," by J. B. Johnson, 1st Ed., 1897, Wiley, New York, N. Y., p. 437.

[47] "Distribution of Stress at the Minimum Section of a Cement Briquette," by E. G. Coker, *Proceedings*, VIth Cong., International Assn. for Testing Materials, Paper XXVIII, 1912.

[48] "Tension Specimens Shape and Apparent Strength," by F. O. Anderegg, Royal Weller, and B. Fried, *Proceedings*, A.S.T.M. 1939, p. 1261.

[49] Experiments on the Strength of Cement," by John Grant, *Proceedings*, Institution of Civ. Engrs., London, 1865–66, and 1870–71; also book of same title, 1875.

[50] "Notes on Cement Testing," by W. P. Taylor, paper before 2d annual convention, Assn. of American Portland Cement Mfrs., December, 1904.

[51] "Data Considered by A.S.T.M. Committee on Cement," Pt. VII, 1916, p. 74.

[52] "Strength of Materials," by S. Timoshenko, Pt. II, 2d Ed., 1941.

Tensile and Flexural Strength of Concrete.—The difficulties of determining the tensile strength of concrete may be demonstrated by data from a paper[53] by H. F. Gonnerman and E. C. Shuman, Assoc. M. Am. Soc. C. E., in which an attempt was made to determine the relation between compressive, flexure, and tensile strength of the same concrete. Table 9 gives compressive strengths

TABLE 9.—COMPRESSION, TENSION, and FLEXURE OF PLAIN CONCRETE[a]

Water-cement ratio	Age at Test				
	3 Days	7 Days	28 Days	3 Months	1 Year
(a) COMPRESSIVE STRENGTH, POUNDS PER SQUARE INCH					
0.68	2,000	3,100	4,710	6,460	7,420
0.76	1,560	2,620	4,460	5,710	6,720
0.87	1,070	1,900	3,560	5,130	6,000
Average...	1,540	2,540	4,240	5,770	6,710
(b) RATIO OF TENSION TO COMPRESSION					
0.68	0.105	0.089	0.081	0.073	0.077
0.76	0.112	0.092	0.081	0.073	0.078
0.87	0.126	0.116	0.089	0.081	0.072
Average...	0.114	0.099	0.084	0.076	0.076
(c) RATIO OF MODULUS OF RUPTURE TO COMPRESSION					
0.68	0.177	0.154	0.135	0.114	0.114
0.76	0.214	0.170	0.135	0.132	0.125
0.87	0.248	0.196	0.164	0.141	0.132
Average...	0.213	0.173	0.145	0.129	0.124
(d) RATIO OF TENSILE STRENGTH TO MODULUS OF RUPTURE					
....	0.54	0.56	0.58	0.59	0.61

[a] Mix, Portland cement to total aggregates was 1 to 4 by volume. Specimens were moist cured and each value is the average of five tests.

of 6-in. by 12-in. cylinders for three water-cement ratios at ages from 3 days to 1 year, together with values for modulus of rupture and tension expressed as fractions of the compressive strength. Tension tests were made by applying loads to 6-in. by 18-in. cylinders by means of heavy cylindrical shackles clamped to the ends of the cylinder. This method is subject to the same criticism as the briquet, in that it does not apply a uniform tensile stress across the section of the cylinder. Flexural tests were made by loading plain concrete beams (7 in. by 10 in. by 38 in.) at the 1/3 points of a 36-in. span.

The modulus of rupture of beams and the tensile strength varied with both water-cement ratio and age; at 28 days the average modulus of rupture was 0.145 and the average tensile strength was 0.084 of compressive strength. According to Mr. Woodard's analysis, all of these values represent Poisson's ratios; but it is seen that the values varied from 0.072 for the 1-yr concrete in

[53] "Compression, Flexure and Tension Tests of Plain Concrete," by H. F. Gonnerman and E. C. Shuman, *Proceedings*, A.S.T.M., Vol. 28, Pt. II, 1928, p. 527.

tension to 0.248 for the 3-day concrete in bending. The flexural strength ratios more nearly correspond to the generally-accepted values for Poisson's ratio.

Particular attention is called to the values in Table 9(*d*), which show that the average tensile strength ranged from 0.54 of the modulus of rupture at 3 days to 0.61 at 1 year. These values correspond closely to the value reported herein under "Tensile Strength of Mortar Briquets," where it was shown that the maximum stress at the edge of a briquet was 67% higher than the average, and indicate that Mr. Woodard's value of Poisson's ratio as computed from the tensile strength of briquets and compression tests of prisms is 67% too high.

Standard Compression Test of Concrete.—Since 1921 American engineers have followed generally the method of compression tests of concrete that was adopted in that year by the American Society for Testing Materials,[54] in which the load is applied directly to the plane ends of the cylinders. For the usual sizes of aggregates, cylinders 6 in. in diameter and 12 in. long are used; for larger aggregates 8-in. by 16-in., or larger, cylinders are used; the U. S. Bureau of Reclamation has used 36-in. by 72-in. cylinders in making compression tests of concrete containing gravel graded up to 9 in.[55]

Mr. Woodard suggests (see "Conclusions") that this method:

> "* * * should be revised to eliminate friction which can be done easily, or a correction factor should be determined to apply to the results of tests as now made."

This suggestion seems to the writer of doubtful merit for several reasons: (*a*) Basing design stresses on the 28-day compressive strength of concrete is purely arbitrary; (*b*) concrete in structures is seldom subjected to the type of compressive stress encountered in a standard test cylinder; (*c*) testing concrete in the form of 6-in. by 12-in. cylinders that have been moist cured for 28 days at room temperature is conventional; quite different strengths would be obtained if other sizes of cylinders or if another ratio of length to diameter were used, or if the age, curing conditions, or rate of loading were changed; (*d*) other factors being equal, the early strength of concrete depends to a large extent on the composition and fineness of the Portland cement.

Effect of Size of Concrete Cylinder.—Compression tests by the U. S. Bureau of Reclamation[55] on concrete cylinders with length equal to two diameters showed the following strength ratios, considering the 6-in. by 12-in. cylinder as 1.00:

Diameter of cylinder, in inches	Strength ratio
2	1.09
3	1.06
6	1.00
8	0.96
12	0.91
18	0.86
24	0.84
36	0.82

[54] "Standard Method of Test for Compressive Strength of Concrete," *A.S.T.M. Standards*, Pt. II, 1942, p. 328.

[55] "Concrete Manual," U. S. Bureau of Reclamation, 1942.

This gives an extreme range from the lowest to the highest of 33%. Doubling the diameter was accompanied by a loss of 7% in terms of the "standard" strength of 6-in. by 12-in. cylinders.

Composition and Fineness of Cement.—The way in which the early strength of concrete is increased by improved compound composition and greater fineness is illustrated by parallel tests by E. Gruenwald[56] on normal and high-early-strength Portland cements of 1938. Typical results are summarized in Table 10. These tests were made on 6-in. by 12-in. cylinders using five sacks of cement per cubic yard, 2-in. slump, and water-cement ratio 0.88, by volume.

TABLE 10.—Strength of Concrete Using Typical Normal and High-Early-Strength Portland Cements

Portland cement	Tricalcium silicate (%)	Surface area (sq cm per g)	Compressive Strength Concrete (Lb per Sq In.)				
			1 Day	3 Days	7 Days	28 Days	3 Months
Normal	47	1,745	480	2,180	3,075	4,675	4,915
High-early	58	2,440	1,795	3,720	4,360	4,915	5,415

An unpublished study of the literature by the writer shows that the average 28-day strength of moist-cured concrete made of American Portland cements, of water-cement ratio 0.80 by volume (0.53 by weight), was about 2,500 lb per sq in. in 1918, but that it increased to 5,000 lb per sq in. in 1940. This resulted from a general improvement in the compound composition of the cement (more uniform burning at higher temperatures) and to finer grinding. This increase would no doubt have continued to the present, had it not been for the lower quality of Portland cement ordered by the War Production Board in August, 1942.[57]

It is well known that the accuracy of the plane surfaces that are exposed to the loading apparatus has an important effect on the compressive strength of stone and similar materials. It was noted by Mr. Merrill,[36] in 1910, that the reported compressive strengths of rocks showed a remarkable increase during 60 years. He properly attributed this to greater refinements in the preparation of the ends of the specimens. However, it does not explain the aforementioned 100% increase in the average strength of concrete, since there have been no significant changes in the methods of mixing, molding, curing, or loading the standard test cylinders. Separate studies of the effect of the compound composition of Portland cement (especially the quantity of tricalcium silicate) and of the fineness of grinding show that the increase of 100% in average concrete strength from 1918 to 1940 can be fully accounted for by these factors. Failure to recognize this notable rise in the "concrete strength level" of Portland cements has been responsible for much confusion of thought, both in scientific studies of concrete mix design and in application of these principles to construction problems.

[56] "Lean Concrete Mixes," by E. Gruenwald, *Proceedings*, A.S.T.M., 1939, p. 810.

[57] "Limitation Order 179, Part 3033—Portland Cement," WPB, August 3, 1942; amended August 24, September 24, 1942, February 16 and March 23, 1943.

Standard Test Cylinders Represent Idealized Conditions.—The foregoing paragraphs show that any strength test of concrete is purely relative and that nothing would be gained by changes in the test method that merely lower the strength by a fixed percentage, such as lubricating the ends of the cylinders, as suggested by Mr. Woodard.

Although the average 28-day concrete strength increased 100% from 1918 to 1940, there was no commensurate increase in the design working stresses in concrete; nor should there have been. The composition and fineness of the cement largely affect the 7-day and 28-day strength of concrete, but they do not exert proportional effects on the strength at 1 year, or 5 years. The normal Portland cement in Table 10 gave a strength at three days and the high-early-strength cement at one day that was equivalent to the 28-day strength of concrete at the time the 28-day strength was arbitrarily chosen as a basis for design stresses.

The compressive strength of concrete cylinders represents idealized conditions that rarely exist in the structure. There are always questions as to whether the concrete in the structure is as carefully placed and whether it receives a curing treatment that will enable it to gain strength, even at 28 days, comparable to that of the test cylinders made under standard conditions. This fact, however, does not minimize the importance of concrete tests made in connection with building operations, since these idealized cylinders do give a measure of the potential concrete quality. Whether this potential quality is realized on the job depends on many factors, most of which are well understood by those skilled in the art. In most applications the strength of concrete must be subordinated to durability. Durability of exposed concrete is more dependent on the use of a plastic mix of a low water-cement ratio than on the early strength. The evidence indicates that the concrete that gave 2,500 lb per sq in. at 28 days in 1918 probably is as durable as the concrete that gave 5,000 lb per sq in. in 1940.

Summary.—The writer maintains that Mr. Woodard:

(1) Completely misinterpreted his briquet tests and in doing so overlooked or ignored information that had been available for nearly 50 years. This mistake of underestimating by about 67% in the true tensile strength of mortars nullifies practically everything that follows.

(2) In his conclusions on greased blotting paper between load surfaces of mortar prisms, he overlooked similar results from using lead, as determined by tests on stone in 1851; and overlooked concrete tests made in 1923 in which blotting paper alone gave the same result.

(3) Mistakenly, he has recommended modifying the standard compression test of concrete to require lubricated ends.

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

DEVELOPMENT OF THE CHICAGO TYPE BASCULE BRIDGE

Discussion

By Alonzo J. Hammond, Past-President and Hon. M. Am. Soc. C. E.

Alonzo J. Hammond,[7] Past-President and Hon. M. Am. Soc. C. E.[7a]—A valuable contribution to the literature on movable bridges is found in this paper. It is especially fitting and appropriate as coming from a bridge engineer who for some thirty years has participated in the design of such bridges in the City of Chicago which has, no doubt, a greater number and a greater variety of these bridges than any other city in the world, due to the required navigability of its two rivers, the Chicago and Calumet.

The writer's introduction to the bridge situation in Chicago was in the spring of 1912 when he was appointed engineer of bridges and harbors, at a time when a large bond issue was available for new bridges, and it was necessary to enlarge the bridge department materially to cope with the demand for new designs and construction. It may be of interest to note that the first problem "dumped into his lap" was a determination of the best type of movable bridge to replace the old swing bridge at Lake Street over the Chicago River. Submitted were two types of vertical lifts, and the Rall, the Scherzer, the Strauss, and what was then known as the City type. On esthetic grounds the vertical lift was eliminated and, on the basis of cost and also esthetic grounds, the City type was adopted.

Previous to 1912, little attention had been given to the artistic appearance of the bridges; as a matter of fact the bridge engineer had to make his dollar give the maximum of strength and stability; but the Chicago Plan Commission came into existence in 1912, so that the opportunity came to the new engineer of bridges to inaugurate the beautification of the new bridges immediately. During 1912 and 1913, the lines of the bridge trusses were simplified and where possible given an arch effect; the masonry abutment, bridge houses, and approaches were built of more permanent material of greater artistic design,

Note.—This paper by Donald N. Becker, M. Am. Soc. C. E., was published in February, 1943.

[7] Cons. Engr., Chicago, Ill.

[7a] Received by the Secretary April 21, 1943.

due to the close cooperation of the engineer of bridges (who during 1912 and 1913 was given a comparatively free hand) with the Chicago Plan Commission and the Illinois Society of Architects. This period may be termed the renaissance in the designing of Chicago's bridges. The immediate results were seen in the Michigan Avenue plans, Lake Street, Chicago Avenue, and Jackson Street bridges. The latter was not designed by the City Bridge Department but the general plans were developed under the writer's direction and approved by him.

Mr. Becker properly has given credit to the long-time employees of the City who were participants in the earlier developments of the movable bridges and who continued over a long period of years to meet the requirements of the traffic successfully. Alexander von Babo was a very able designer, modest and retiring, who gave freely to the City his patented ideas on which he reaped little or no financial reward.

A movable bridge is a large machine which requires constant attention for successful and satisfactory operation. For one thing, power is important, so there was installed in 1912–1913 duplicate sources of supply of electric power. One of the early difficulties in the Scherzer type of bridges was the size and shape of the teeth on the horizontal rack, and a more effective design was developed to assure a more safe meeting of the demand. As a matter of fact the Canal Street bridge almost "walked into the river" one night; this possible situation is now avoided by the better designing.

Under the heading, "The Improvement Period," Mr. Becker refers to the Chicago Avenue cylindrical piers as founded on clay at El. − 82.0, when, as a matter of fact, this is bedrock, as shown in Fig. 11. Bids were received on piers stopping in the hard clay with cone shaped bottoms, flaring out to give a sufficient spread to take the loads, and also on cylindrical piers extending to rock.

It so happened that the less excavation and less concrete made the cylindrical piers cheaper, but it gave a star reporter on the leading morning Chicago paper a chance to write an article about political favoritism to the successful bidder, the article appearing on the front page in large type. Later the said star reporter denied the truth of the article, but this was buried on the inside pages of the paper.

One day the Mayor, Carter H. Harrison, Jr., came into the office and complained about the shock he got going over the center break in the floor of the State Street bridge. This was a very light structure, so the complaint led to the designing of the continuous-rail joint at both center and heel breaks, which were adopted against the objections of the supervising engineers and have proved to be a great asset.

As a matter of interest to those bridge engineers who may be designing double-leaf bascule bridges, the following incident happened in the case of the West Lake Street bridge shown in Fig. 12: When the writer took charge of the bridge department he found borings had been made on both sides of the river at Lake Street, showing rock at El. − 75.0. Suspicious that the elevations should be the same, where there was a possible dip, new borings were ordered, and at El. − 75.0, sure enough, rock was found, but only 6 in. in

thickness—so the borings were continued down until bedrock was found at about El. − 113.0, as shown in Fig. 12.

Mr. Becker refers to a new idea as developed in the Madison Street bridge, in 1922, called the "railing height truss." As a matter of fact two designs were prepared in 1912 for the Jackson Street bridge, one a railing height truss and the other a deck span, and the writer approved the latter. At about the same time, preliminary measurements and studies were made under his direction for the Madison Street bridge, and these preliminary sketches were for a railing height truss. It will be of interest also to note that a temporary bridge was designed, at this time, of the retractile type to carry traffic across the river during the construction of the proposed new bridge. This was a very simply designed structure, as railroad tracks were laid at right angles to the dock on vacant ground at dock height. The design included car trucks, on top of which were trusses extending far enough to the rear to carry a counterweight and far enough in front to provide a span of about 60 ft—a balanced affair planned to run out and back. It was not used, however.

The Michigan Avenue double-deck bridge was designed under the writer's direction in 1912 and 1913 and had a novel history—the construction was paid for by property assessments, and the drawings had to be reduced. The latter, with the specifications, were made a part of the council proceedings and were published in detail. A close examination of Fig. 18 will show the Michigan Avenue bridge in the background—a bridge that carried an enormous automobile and bus traffic until relieved by the outer drive bridge.

Since 1912 river traffic has dwindled tremendously and the need of movable bridges has seemed to be approaching an end; but with the present war some bridges that were not in operation have been provided with operating machinery by the federal government; so it appears that the movable bridge is in for a long lease of life on the Chicago River.

The designing engineer of Chicago's bridges had to meet two rigorous conditions which taxed his designing ability to keep within limits. The grades on the approaches were kept to a maximum of 4% and the under clearance above river level was required to be not less than 16.5 ft for a certain width of space. The type of bridge to meet these conditions was a two-leaf bascule, and that which is known as the City type has proved a very satisfactory design.

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DISCUSSIONS

THE QUEENS MIDTOWN TUNNEL

Discussion

By Messrs. Francis V. Wagner, William J. Wilgus, and S. A. Thoresen

Francis V. Wagner,[9] Jun. Am. Soc. C. E.[9a]—The relatively meager literature on subaqueous tunneling has been well supplemented by this valuable and comprehensive paper. Inasmuch as its very scope precluded extensive treatment of any one phase, the writer wishes to enlarge on one aspect of the job. The author mentions the unfavorable conditions for compressed air tunneling that existed in the extremely porous ground beneath the Manhattan River bulkhead. When the heading was in this region, the consumption of compressed air increased to an alarming rate, and the bill for electric power to supply it reached the relatively high average of $4,900 per week. This sum, of course, is the result of an unusual situation. A weekly electric bill of $1,750 would be more representative of average conditions. While he was an employee of the New York City Tunnel Authority, a study was undertaken of power-house costs for the construction of the Queens Midtown Tunnel.

Functions of the Power House.—As stated by the author, the primary function of the power house is to supply sufficient air to the heading to maintain the desired pressure. This is known as "low air." However, the power house has several other functions: It must supply compressed air to operate tools and machinery in the tunnel. This is known as "high air" and, since these tools and motors exhaust into the heading, it must be at a pressure of 60 to 80 lb per sq in. greater than that existing in the heading. The power house must also supply sufficient water under a pressure great enough to operate the hydraulic jacks that propel the shield. It must supply direct current for charging the batteries of the electric locomotives used in construction operations and for miscellaneous power.

Power-House Costs.—The total power-house costs will be the sum of the fixed charges for the entire power house, and the operating costs for each of its functions.

Note.—This paper by Ole Singstad, M. Am. Soc. C. E., was published in March, 1943, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: May, 1943, by Messrs. Thomas W. Fluhr and Howard L. King.

[9] Senior Structural Engr., Brewster Aeronautical Corp., Johnsville, Pa.

[9a] Received by the Secretary April 22, 1943.

The cost of the land is the first fixed charge that must be considered. If the land is leased, the rent will be a fixed charge. If it is bought, taxes will be in the same classification. The largest charge, of course, is the amortization of the original investment and interest on the capital tied up in it. Although this actually might be done by any of several methods, for the purposes of this study it was considered that this charge was distributed evenly over the entire period during which the power house was supplying low air to the tunnels. Therefore, it was constant.

The cost of insurance, of course, was constant over the period to be studied. Although the charges for light, heat, and incidental maintenance to the building varied seasonably, their relative weight was negligible, and it was considered sufficiently accurate to compute them as fixed charges.

Operating Costs.—The operating costs of the individual functions are composed of the charges for power, and for labor for both operation and maintenance. Some of these costs will be constant and others will be variable.

The cost of power will vary according to the amount of the particular item supplied. The cost of labor will vary according to the amount of man-hours necessary to supply the item. It will not vary, necessarily, with the amount of the item supplied. The usual force in the power house was an engineer, an oiler, a gage tender, and a hydraulic maintenance man. This force was sufficient for all operations and for all ordinary, routine maintenance. It devoted a certain amount of time to each function—supplying low air, high air, hydraulic pressure, and electric current. The fact that the high air compressors were running at three-quarters speed and the low air compressors at half speed, or vice versa, did not, necessarily, alter the distribution of their time between these machines. No records were kept of the actual time distribution of these men. Inasmuch as they were responsible for the entire power house, and distributed their time as they saw fit, it was impossible to allocate this labor cost to each function. Therefore, it was considered as a constant charge.

The only variable charges therefore were the costs of power to supply the various functions of the power house. To consider them in the inverse order of their importance: Direct current was supplied for charging the batteries of the electric locomotives and for other miscellaneous power. When the job was underway, the amount consumed was a fairly constant one. No matter what the type of work in the tunnel, the electric locomotives were kept busy hauling materials and muck, and, consequently, needed recharging at a fairly constant period. Direct current consumption for other miscellaneous power was small and may be considered to be constant. The error, accordingly, will be negligible, if the cost for supplying direct-current power be considered a constant charge.

Variable Costs.—Hydraulic power must be supplied to actuate the various jacks on the shield. This water was stored in two hydraulic receivers, which were kept charged by three 100-hp high-pressure hydraulic pumps. The quantity of water used varied with the number of times the jacks were used. With the exception of a few times when the smaller "face" or "platform" jacks were used, jacks were used only during a shove. The cost of supplying this hydraulic power, therefore, varied with the frequency of shoves. This water was pumped

continuously, however, and the amount of power consumed by the pumps was small in comparison with that used for the air. There were two other variable costs: The power to supply high air and the power to supply low air.

High Air Consumption.—An important function of the power house was to supply high air to operate power tools all over the job. Some of this was used on the surface and outside the compressed air chambers and was exhausted to the atmosphere. Much of it was used in the heading, and was exhausted therein, thus augmenting the supply of low air. This quantity, then, was used twice—once as high air for power, and once as low air. Although the amount that was thus doubly utilized varied, on the average it is believed that it amounted to about 70% of the high air compressed.

The high air used on the surface powered various tools in the machine shop and yard. Routine maintenance and repairs required a relatively constant amount. However, when there was much rock in the heading, the sharpening of drill bits increased the consumption of high air in the machine shop.

In the heading high air was used for power for a monorail hoist and for various "tuggers." Inasmuch as these machines were used in routine fashion, the amount of high air consumed in their operation was assumed to be constant. The power wrenches for tightening the bolts in the tunnel lining consumed some high air. The amount used in this manner was governed by the frequency of ring erection, which, of course, is the same thing as the frequency of shoves. This was a very small amount. Somewhat larger consumers of high air in the heading were the power spades and the rock drills; but the largest users of high air were the grout and gravel machines. These were used most, when the heading was in rock. The high air consumption in the tunnel might be expected, accordingly, to vary with the amount of rock in the heading.

The only important factor that might be expected to govern the variation of the cost for supplying high air is the amount of rock in the tunnel heading.

Low Air Consumption.—The amount of low air consumed was equal to that which escaped from the tunnel. As has been noted, the air in a tunnel can escape by four paths. The quantity that escapes by all of those ways is, of course, directly proportional to the pressure existing in the tunnel at the time. The amount that escapes by each of these methods, however, is determined by several other factors as well.

The first way in which low air can escape is by means of the air locks through which men and materials enter and leave the tunnel. The amount lost in this manner is dependent upon the number of times the lock is used.

The second avenue of escape for the low air is through the joints in the iron lining. The volume that leaks through these joints is dependent upon the tightness of the joints; but once through the joints, the air must escape through the ground surrounding the tunnel. The porosity of this ground, therefore, is also a governing factor.

The third way in which this air can escape is through the annular opening between the tail of the shield and the last completed ring of lining. The quantity that escapes in this manner is dependent upon the condition of the opening. It is usually kept "bagged up"—that is, stuffed full of old burlap bags—which reduces the leakage considerably. Again, as in the case of joint

leakage, the air must escape through the river bed, and that being the case the porosity of the ground is the determining factor.

The last path of escape for the low air is through the face of the heading itself. Once more it is evident that the condition of the opening governs the volume of air that escapes. If the breasting boards are tight and well packed with bags and salt hay and plastered with clay, leakage will be less than through a more open face. As in the last two methods of escape, however, the air must leak through the ground itself. The character of the soil, then, is once more the dominant factor in any study of the volume of leakage.

It must be evident from the foregoing that any study of the factors governing the consumption of low air must take considerable account of the type of ground traversed by the tunnel. With the exception of that volume lost through the air locks, all the low air consumed escapes through this ground. A consideration of the size of the openings through which the air leaks would lead to the conclusion that most of it escapes in the vicinity of the shield. This view is confirmed by observation of the river surface during the construction of the tunnel. Directly over the shield the surface of the water was in constant agitation due to the escaping air. The limits of the area in which bubbles of air were breaking the surface could be noted plainly from a rowboat, and were even more clearly defined from an elevation. Usually, elsewhere along the line of the tunnel, however, no air leakage was discernible at the surface. It is safe to assume that most of the air that escaped through the ground did so near the face of the heading. The type of ground in the heading was one of the most important factors that governed the consumption of low air. It is to be expected, therefore, that, the other factors being equal, if the heading were in impervious material, for instance, the amount of low air consumed would be less than when the heading was in loose ground.

Factors Influencing Variable Costs.—It has been shown, then, that the variable costs for the power house depended, in the main, upon the consumption of high and low pressure air. The factors which governed such consumption would govern the variable costs. Accordingly, the factors that influenced the variable costs of the power-house operations may be listed. They included the air pressure in the tunnel, the frequency of use of the air locks, and the type of ground in the heading.

Statement of the Problem.—Inasmuch as these variable costs combined with certain fixed charges to comprise the operating costs of the power house, it follows that the operating costs of the power house are dependent upon the same factors. In the end, these factors make themselves felt upon the total costs of power-house operations. However, the influence of these factors may be great or it may be small. The first question which must be answered, therefore, is:

1. What is the extent of the variation in operating costs with the factors which influence them?

Once this question has been answered, a second one presents itself:

2. Is this variation in operating costs of sufficient magnitude to affect the total costs of the power house to any appreciable extent?

Should the answer to question 2 be in the affirmative, a study can be made of the variation in total power-house costs with the factors that influence them.

Method of Attack.—In order to answer questions 1 and 2 for the Queens Midtown Tunnel, a survey was made of the power-house facilities which were set up for the construction of the tunnel. That the headings which were driven from opposite sides of the river might be served, the contractor established a power house for each of the two construction shafts. The operations of the Manhattan power house were highly irregular, and fluctuated considerably due to several blows and fires which interfered with the prosecution of the work, and due to many sudden variations in the character of the material in the heading. On the Queens side, however, operations were fairly constant for considerable periods, and conditions were favorable for the study proposed. The Queens power house was chosen as a basis for the study. All information contained in this study and all data presented herein were obtained from the records of the Queens Field Office of the New York City Tunnel Authority.

First, a profile of the tunnel, similar to Fig. 11, was drawn, showing the variations in strata through which the face of the heading passed. On this was superimposed a progress chart of construction, which fixed the dates during which various conditions of face prevailed in the heading. From a consideration of the profile, and from a study of the Queens power-house records, certain periods were chosen which isolated each of the aforementioned variable factors. Then all the operating data of the powerhouse were compiled into usable form for these selected periods. From a study of such data concerning the isolated factors, the extent of their influence upon the quantities involved and, therefore, upon the operating costs, was determined. The first question then was answered.

Once the influence of the governing factors upon operating costs was known, their influence upon total costs could be discovered, and the answer to the problem could be found.

Running Speed of Compressors.—The study disclosed that the plant rarely operated at more than one third of its full capacity of 64,152,000 cu ft per day. To obtain this quantity of low air, the compressors were run alternately, usually at one-half or three-quarters load, which was not the point of their highest efficiency. Occasionally, however, for short intervals, they were run at full load. It was this peak load which determined the power cost, and, at this peak, the machines were operating at their highest efficiency. The efficiency at which they operated for the remainder of the time was of no importance. In order to avoid all chances of a breakdown, then, they were run most of the time at the lower speeds.

Summary.—It was concluded from the study that, in the case of the Queens Midtown Tunnel, there was no appreciable variation in the quantity of high air compressed. No correlation could be discovered between the quantity of low air consumed and the frequency of use of the air locks, or between the quantity consumed and the pressure used in the heading. Nevertheless, the quantity of low air consumed varied directly with the porosity of the ground in the face of the heading, as measured by the pressure used therein. This is

shown in Fig. 13. The unit operating costs of the power house decreased in such proportion to the quantity of low air supplied that the operating costs of the tunnel were constant while it was supplying between 12,000,000 and 20,000,000 cu ft of low air a day. This is shown in Fig. 14.

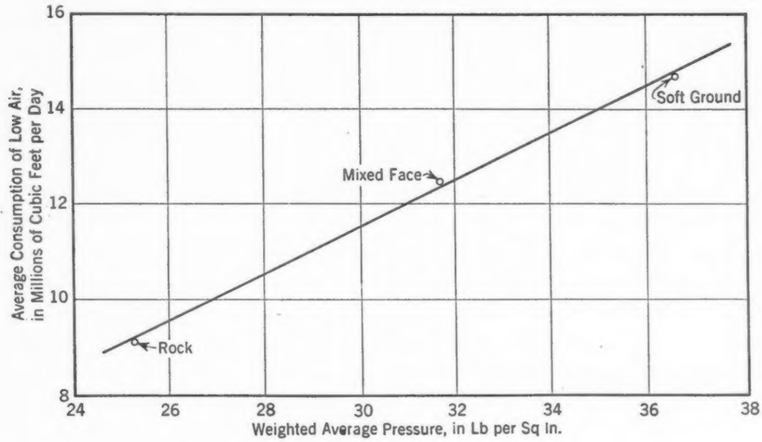

FIG. 13

Power-House Costs.—The total daily power-house cost was constant at $928.27 a day. On the basis of this value, the various elements which constitute it represent the following percentages of the total:

Item	Daily cost		% of total daily cost	
Land	$ 56.563		6.1	
Interest	1.107		0.1	
Amortization	442.638		47.7	
Insurance	6.640		0.7	
Total fixed cost		$506.95		54.6
Labor	178.84		19.3	
Power	242.48		26.1	
Total operating cost		421.32		45.4
Total daily cost	$928.27		100.0	

Trends and Indications.—For the Queens Midtown Tunnel the daily fixed charges were about 20% greater than the average daily operating cost. It would require a fairly large percentage variation in operating costs to cause any appreciable percentage of variation in total power-house costs. This indicated that it is the fixed charges which are important in considering construction power-house costs, and that savings made in them are of more importance than economies in operating expenses. It must be emphasized, however, that the Queens side of this tunnel was in exceptionally good ground for

tunneling. It consisted of very fine sand and clay which was not at all porous. In soft ground, therefore, air consumption was relatively low, and, what is more, peak loads were infrequent and were not excessively greater than the average.

Inasmuch as power costs could have been driven sharply upward by an unexpected peak load, the question of maintaining as even a power demand curve as possible is of paramount importance. It must not be forgotten, however, that the low air is a tool that must be used efficiently. Sometimes peak loads are vitally important for economies elsewhere in the work, and then

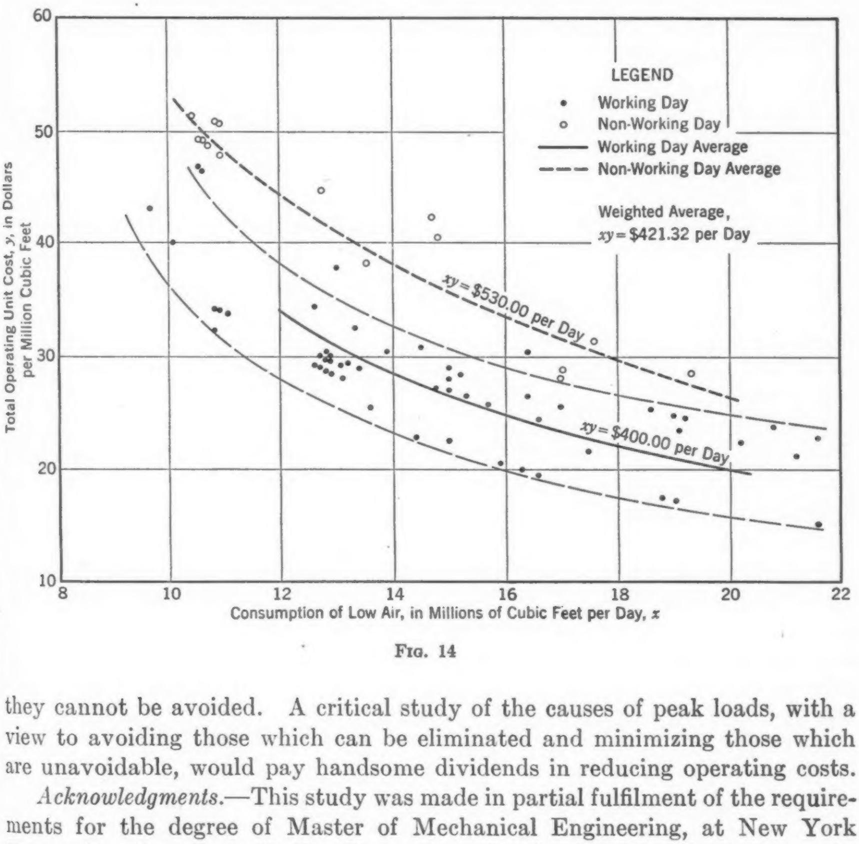

Fig. 14

they cannot be avoided. A critical study of the causes of peak loads, with a view to avoiding those which can be eliminated and minimizing those which are unavoidable, would pay handsome dividends in reducing operating costs.

Acknowledgments.—This study was made in partial fulfilment of the requirements for the degree of Master of Mechanical Engineering, at New York University, New York, N. Y. The writer is indebted to Prof. A. C. Coonradt, head of the Department of Mechanical Engineering, for his advice and encouragement, and to numerous engineers of the New York City Tunnel Authority and of the Walsh Construction Company, without whose cooperation the study could not have been made. The results were compiled into a thesis: "A Study of Powerhouse Costs for the Construction of the Queens Midtown Tunnel," which is on file in the Library of New York University, and of which the present discussion is a brief summary.

WILLIAM J. WILGUS,[10] HON. M. AM. SOC. C. E.[10a]—This is indeed an admirable account of the birth of a star of the first magnitude in the galaxy of great engineering feats that grace the nation's most populous region. Seven monumental bridges for vehicular traffic now span the wide tidal waters encompassing the island of Manhattan on the east and west and between the Bronx and Queens; and, beneath the surface, pass three subaqueous tunnels that serve the same purpose, the one described in the paper being the latest. Between lower Manhattan and Brooklyn a fourth tunnel is in the making, and, beneath the Narrows between Brooklyn and Richmond, a fifth is in contemplation by a method differing from the others. It is to be hoped that more papers will come from the pen of the author, giving the profession the benefit of his remarkable experience in planning, constructing, and operating such public works.

The writer is interested particularly in the happy result that attended the freedom given the bidders, in the case of a part of the Queens Tunnel, to make a selection from specified alternative methods of construction, or, if they preferred, to offer their own solutions on which to base their bids. It happens that the writer took occasion to do this many years ago in the preparation of the preliminary plans and specifications both for the construction of the subaqueous section of the Detroit River railroad tunnel, which resulted in the adoption of his trench-and-tremie method, and for its electrification, which resulted in the selection of the direct-current, third-rail method. Jokingly, this course was referred to, by one of the tunnel bidders, as a "brain poultice," as was true. In a novel situation the bidders, governed by the determined location, gradients, dimensions, and qualities of materials and workmanship, thus are given an opportunity of basing their proposals as to time of performance and costs on methods they consider best qualified for the achievement of the desired end; and the principal is given the assurance of a successful outcome at a minimum of first cost and cost of operation.

In his closing remarks, perhaps the author will give comparisons of leakages and costs per cubic foot of internal content of the subaqueous sections of the Holland, Lincoln, and Queens tunnels—with an index of costs of labor and material predicated on a scale of 100 in the first-named instance. It would be a matter of interest to compare these costs with those for similar work in the past and a help in making rough preliminary estimates for like work in the future.

The inclusion of interest on money during construction in the author's statements of cost is of great significance; too often, this is omitted from estimates of the cost of public work to the injury of the taxpayer who thus is misled into believing that his burden will be far less than truth demands. Also, it is gratifying to know that the actual cost of this project was less than the estimated amount. The question may well be asked if the latter is the one that obtained at the outset or was prepared at a later date after unforeseen conditions and difficulties had developed. In public work a suitable provision for contingencies too often is omitted at the beginning because the enlarged

[10] Weathersfield, Ascutney P.O., Vt.

[10a] Received by the Secretary April 23, 1943.

figure may scare the public and discourage approval of the project. Utter frankness in these respects is due those who are to pay the bills, as has been shown so obviously in the paper.

In addition to the purpose of the tunnel as a utility, it is evident that sight has not been lost of the esthetic. The interior treatment is most attractive, and the facing of the walls of the approaches with stone gives to them a sense of solidity and comeliness as befits the dignity of a great public enterprise.

S. A. Thoresen,[11] M. Am. Soc. C. E.[11a]—The presentation of this valuable contribution reveals the principles of design and construction details that were produced by a staff of engineers with long experience in a specialized field of engineering and construction. The development of vehicular tunnels, being closely related to the increased use of motor vehicles, may be considered a twentieth century product of research and invention. When comparison is made of the modern type of construction with that of Blackwall Tunnel in England, built before the introduction of the automobile, it is apparent that careful thought has been given not only to invite the traveling public for a ride through "a hole in the ground" but also to enjoy an attractively finished, well lighted, pleasant, and comfortably ventilated subaqueous highway.

The construction of the Queens Midtown Tunnel through rock of variable character, with overburden of sand, gravel, and numerous boulders, presented a most difficult problem, and the engineers and contractors entrusted with its execution merit due credit.

Reference has been made to this tunnel as being the first tunnel crossing of the East River for automotive vehicles. In connection with this statement it may prove of interest to review certain historical facts dating back to the beginning of the century, when the first subaqueous rapid transit crossings under the East River were projected and built under the direction of the late William Barclay Parsons, Hon. M. Am. Soc. C. E.

The completion of the Battery Tunnel from South Ferry, Manhattan, to Joralemon Street, Brooklyn, inaugurated the first rapid transit service under the East River, to be followed a few years later by the construction of the Steinway Tunnel from 42d Street, Manhattan, to Long Island City, Queens.

Since the Steinway Tunnel route is in close proximity to that of the Queens Midtown Tunnel, the records kept during the period of construction show certain similarities with respect to the topography of the rock floor and to the deposits of sand, gravel, and boulder formations. On the Manhattan side the tunnel was driven through solid rock and the river section was driven from three shafts, one located at the foot of 42d Street, a second in the middle of the river, and the third in Long Island City. Rapid progress was made by attacking the river part from four faces, and the completion of the twin-tube tunnel during a two-year period from 1905 to 1907 reflects credit on the pioneer engineers and tunnel contractors of that time for a successful and speedy solution under difficult ground conditions full of hazards.

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[11] Cons. Engr. (Parsons, Brinckerhoff, Hogan & Macdonald), New York, N. Y.

[11a] Received by the Secretary May 15, 1943.

The enterprising spirit and economical prosecution of work in those days (when money was subscribed from private sources) are best demonstrated by actual records, when, in 1906, labor cost for rock excavation under 35-lb air pressure would average from $6 to $7 per cubic yard and the contract cost for this twin-tube rapid-transit tunnel from Grand Central Terminal to Long Island City, 8,500 ft long, was about $5,000,000. The cost of contracts as given by tabulation in the paper is of interest in showing the tendency toward an increase in cost of tunneling under present-day labor, material, and overhead charges, as compared with work performed years ago or during the economic depression. Any combination of construction methods that may reduce costs to a lower level will be accepted favorably, particularly when the total invested capital is supported by toll collection.

Since the construction of the Holland Tunnel (when ventilation research and experimental work were conducted on an extensive scale in collaboration with the U. S. Bureau of Mines), its ventilation system—namely, fresh air beneath the roadway and vitiated air above the ceiling slab—has been adopted in principle by all modern vehicular tunnels.

In Table 1 it is interesting to note the economy of operation brought about by introducing large and small motors for full and partial load conditions as a means of reducing power consumption according to traffic.

Any improvements that tend to reduce the construction cost, and correspondingly the fixed charges must be borne in mind by the tunnel engineer, if the subaqueous highway is to become the means for linking together cities, states, countries, or continents, as the case may be, thus meeting the demands of the future in competition with other methods of transportation.

Postwar problems may well include projects of this character which, in combination with the trench and pre-cast type of design, possess certain economical advantages where soil conditions may favor types of construction such as those adopted for the tunnels under the Detroit River or the Oakland-Alameda Estuary. The latter type of construction has not proved practical for tunnels projected under the Hudson and East rivers but, without doubt, will receive proper consideration in connection with other plans within the New York metropolitan area.

As a monumental structure, the Queens Midtown Tunnel is another contribution to the many underground facilities of Greater New York and, like all great achievements, is an example of organized talent dedicated by the engineering profession to the service of the general public.

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

FLOW CHARACTERISTICS AT RECTANGULAR OPEN-CHANNEL JUNCTIONS

Discussion

BY HAROLD K. PALMER, M. AM. SOC. C. E.

HAROLD K. PALMER,[5] M. AM. SOC. C. E.[5a]—Some interesting experiments are offered in this paper in an attempt to find a solution of some important hydraulic problems; but unfortunately some of the author's hypotheses are not confirmed by the experiments, which leads to the suggestion that the plan of attack may require some modification.

An examination of the photographs in Fig. 4 shows clearly that there are three zones at the junction: A zone of slight deflection, and the narrowing of stream 1 along the straight side; a bending and narrowing of stream 2 around the angle; and a zone of turbulence between the two outer zones, where the mixing of the two streams takes place. The deflection of a stream will cause a small loss of energy and in the zone of turbulence there will be a much larger loss, but the two are of such different natures, and affect such different parts of the same stream, that they should be considered as two terms with different coefficients. The shape of the junction would have a marked effect on the loss due to bending stream 2 and might affect somewhat the loss due to turbulence. It appears to the writer, therefore, that the problem should be approached from the standpoint of loss of energy rather than from change of momentum.

To solve the problem by this method it would be necessary to assume y_3 and v_3 below the junction of the converging streams, thus establishing the relation:

$$E_3 = y_3 + H\,v_3 \qquad (7a)$$

in which E represents the total energy in the part of the stream indicated by the subscript.

Stream 1 would lose some energy in the zone of turbulence and in being confined to a narrow strip, and it would require some experiments to evaluate

NOTE.—This paper by Edward H. Taylor, Jun. Am. Soc. C. E., was published in November, 1942, *Proceedings*.

[5] Office Engr., Los Angeles County Sanitation Dists., Los Angeles, Calif.

[5a] Received by the Secretary May 17, 1943.

this combination of losses. Once this loss is found, the energy in this stream would be expressed by the relation

$$E_1 = E_3 + \Delta E_1 \qquad (7b)$$

In the same way,

$$E_2 = E_3 + \Delta E_2 \qquad (7c)$$

In the latter case, the loss due to deflection would be much greater than in stream 1, but still would be much less than the turbulence losses.

To find the constants to be used in this method, it would be well to conduct experiments on the deflection of a single stream, avoiding the turbulence losses. The only way to segregate the deflection and turbulence losses is first to find the magnitude of one and then subtract that from the total to find the other. Such a study may determine the importance of rounding corners on the concave side of the entering stream to avoid excessive loss, and it would also show the effect of the width of the side channel.

The same methods could be applied to the case of dividing flows. In this case, the deflection loss is very important, as the momentum of the water tends to make it continue down the straight channel. The width of the side channel is even more important in this case and, if the side channel can be so designed that the stream is always in contact with the convex bank, there will be no backwater, and losses will be minimized.

This proposed method of attack may not be the solution, but the writer believes it accounts more nearly for the forces acting on the streams, and that therefore some constants may be found that would give an approximate idea of what happens in either combining or dividing streams.

If such studies should prove successful, it would not be necessary to construct a model for every case, as suggested by the author. It is to be hoped that the continuing studies at the University of California, which the author mentions in his "Conclusion," may help to solve this general problem by furnishing the necessary constants.

Correction for *Transactions:* In November, 1942, *Proceedings*, page 1521, delete lines 14 and 15 and footnote 1*a*.

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DISCUSSIONS

DEWATERING, INCINERATION, AND USE OF SEWAGE SLUDGE

A SYMPOSIUM

Discussion

By Willem Rudolfs, M. Am. Soc. C. E.

Willem Rudolfs,[9] M. Am. Soc. C. E.[9a]—Adequate preparation and proper disposal of sewage sludge continue to be among the principal problems of sewage treatment. In general, the processing of sludge for fertilizer or burning requires that the moisture content of primary, digested, or activated sludge must be reduced as much as possible by chemical and mechanical means. Dewatering and air-drying of sludge on sand beds continue to be practiced to a considerable extent, especially where beds can be used throughout most of the year, where natural sand beds are available, for small plants where considerable land but no technical skill is available, and because the method is economical.

As a rule, dewatering of sewage with the aid of chemicals by means of vacuum filtration is not advisable for small plants, except where local conditions such as area available, proximity to dwellings, and final disposal of the sludge require special methods. Nevertheless, there are a number of small plants utilizing vacuum dewatering without any apparent need. The cost is frequently out of proportion and the operation not as good as efficiency and economy demand.

Sludge pressing with the aid of chemicals is used in a few cases; one plant dries its digested sludge successfully by spraying; but preparation for dewatering by cooking, or by direct dewatering by centrifuge, has not been adopted in the United States.

Dewatering.—Most of the dewatering by vacuum filtration is restricted to larger plants. The several Symposium authors are in agreement that for most efficient practice certain requirements are essential:

1. The sludge should be concentrated as much as possible before filtration;

Note.—This Symposium was published in January, 1943, *Proceedings*. Discussion on this Symposium was published in February, 1943, *Proceedings*, by A. L. Genter, M. Am. Soc. C. E.

[9] Chf., Dept. of Water and Sewage Research, New Jersey Agri. Experiment Station, New Brunswick, N. J.

[9a] Received by the Secretary April 27, 1943.

2. Operation of the filters should be as nearly continuous as possible;
3. A conditioning agent must be used;
4. The conditioning agent must be added in accurate proportions and thoroughly mixed with the sludge in the shortest possible time;
5. Filtering must proceed as soon as possible after mixing, to take advantage of the floc formed; and
6. Flexible machinery and close control are essential.

The present method of vacuum filtration is an adaptation of industrial dewatering of slurries. The mechanical structures have not changed greatly, although some minor modifications have been made. The crux of the vacuum filtration method has been, and still is, the proper type and use of chemicals for conditioning. The type of chemical used at present is limited to iron and aluminum salts, with or without lime. Selection of the chemical has been determined by local conditions, availability, character of sludge, etc., but not always on scientific grounds. Under all conditions good coagulation is necessary to avoid troubles. Although great advances have been made in the knowledge of the behavior of sludges and the action of chemicals, much remains obscure. Operating experience at Buffalo, Minneapolis-St. Paul, Chicago, and Washington, D. C., indicate developments. As filter medium, Canton flannel or woolen blankets are used. Difficulties with clogging by iron or lime can now be overcome, but clogging of the filters by thin jelly-like sludge occurs frequently at smaller plants with less efficient operation. Lime scale is removed successfully from filter screens, troughs, and piping with the aid of muriatic acid, at Buffalo; but such removal has failed at other places.

Thorough distribution and rapid mixing of the conditioning chemicals in the shortest possible time are stressed by all authors. Under certain conditions mechanical stirring appears to be favored over air-mixing: Mr. Schroepfer has improved air-mixing facilities by increasing the number of points by which air is admitted, thus reducing the violence of mixing; Mr. Dundas states that most devices available for mixing, although theoretically correct, fail in practical operation on account of their complexity and inability to withstand the action of the chemical; and Mr. Velzy reports that at Buffalo the tendency is to stay away from mechanical appliances for the application of chemicals. Although all agree that rapid mixing and application of chemicals are necessary, there seems to be no definite agreement on the procedure. Practices in a number of small plants vary much more widely than indicated by the Symposium authors. It is questionable whether standardization of practice is desirable or needed.

Some problems requiring further study are indicated. Information is required as to the reaction between coagulants and sludge ingredients; the effect of greases and soaps on the rate of filtration; the effect of thickness of cake on yield; filter washing to prevent binding and reducing the life of cloth; removal of scale and incrustations; type of filter cloth for different sludges; sludge feeding methods to the filters to obtain even loading at all times; improvement of dispersion of chemicals (riffle boards, perforated pipes, ejectors, pumps, sprays, etc.).

The operating cost for conditioning and vacuum filtration as given by the different authors varies greatly. The low figure of $2.19 per ton of dry solids at Minneapolis-St. Paul for primary and chemical sludge is outstanding. Washington, with primary elutriated sludge at $4.64 per ton dry solids, is near the average and somewhat higher than the Chicago range for primary and activated sludge of $3 to $4 per ton of dry solids. Cost data published from a number of small plants, where flows are treated below 10 mgd, vary from $8 to $20 per ton, some being even higher. To these costs must be added the expense of handling and disposal of the cake by dumping, fertilizer production, or incineration.

Drying and Incineration.—Difficulties with drying and burning of the sludge appear to be less than those encountered in dewatering. Uniform feed and continuous operation are stressed to produce maximum surface exposure of the sludge particles in drying and to prevent rapid changes in furnace temperatures. The usual practice of returning previously dried material, which is agitated and mixed with dewatered sludge, appears to have reduced damage, prevented scorching, and alleviated odors. There are still some difficulties. Abrasion of flash dryer fans and pipes had been severe at Buffalo, which was reduced by the use of gunite; nevertheless, "it has constituted a problem calling for careful attention." Removal of the preheater in one of the incinerators resulted in considerable savings in power and maintenance cost at Minneapolis-St. Paul. The odor problem of Chicago was solved with electrical precipitators. Constant control, close supervision, a considerable degree of technical and directive ability, skilled labor, and a high degree of interest of the staff are required for economic operation and maintenance.

It is of considerable interest that the cost of dewatering the sludges appears to be from one and one-half to two times the cost of incineration. The total operating cost reported for disposal of the sludge varied from $3.53 to $7.69 per ton of dry solids. Comparison of these costs with those available for barging primary and activated sludges, which varied for the same years from $2.88 to $4.00 per ton dry solids, indicates the present advantage of cities located near the sea. There appears still to be room for improvement in drying and incineration; but since most of the difficulties and the higher cost occur in the conditioning and dewatering of the sludges, studies on simplified dewatering, sludge concentration with, or without aid of chemicals, to a sufficient extent to allow direct incineration, may lead to fewer difficulties and lower cost.

Fertilizer.—For years far-sighted soil scientists have deplored "the wealth that flows to sea." It has been estimated that sewers in the United States carry away enough nitrogen to fertilize 1,000,000 acres of farm land. Although some of the larger cities realize the possibilities, and are considering more extended use of sludge as a soil conditioner, numerous small plants have had material available, which constitutes, in Mr. Van Kleeck's words, "an economic waste of a valuable natural resource."

The use of raw primary sludge as a fertilizer is not recommended for a number of reasons given by Mr. Van Kleeck. The principal objections are odor, grease, acidity, pathogenic organisms, and insufficient humification. Unquestionably, these objections are valid when the material is applied in the raw

state to truck-garden crops or used around dwellings. However, the utilization of raw sludge as a soil builder or fertilizer may be reconsidered if the material is to be used by farmers who may store and compost it. Raw sewage sludge mixed with soil, plant residues, or manure and allowed to decompose aerobically for a number of months makes an excellent material of an equally good or better grade than digested sludge. All objections raised disappear because such composted material has an earthy odor and contains a minimum of grease; acidity has been destroyed; pathogenic organisms are killed; and humification has taken place. The rate of decomposition in compost piles is high and naturally varies with the seasons. The heat produced is material even during the colder season.

Although any type of sludge has some fertilizer and fertility value and is capable of improving soils, especially the moisture holding capacity, its value should not be overemphasized. With the present great demand for soil-building material in "Victory Gardens" it has become a question of restraining rather than stimulating the use of sludge. Thousands of people who had no gardens before turn to the "wealth that flows to the sea" and believe that a bag of sewage sludge automatically insures abundant and luscious crops. Disappointment may lead to a wholesale condemnation of the use of sewage sludge. On the other hand, with restrained recommendations, the use of sludge for agricultural purposes may be greatly increased after the emergency is over.

The chemical composition of digested sludge varies materially from place to place. Analysis of many hundreds of samples collected all over the United States shows the following percentage ranges and averages on the basis of bone dry material:

	Range	Average
Volatile matter	42 –64	51
Total ash	58 –36	49
Grease and fats	3 –17	8
Total nitrogen (N)	1 – 4.5	2.2
Phosphorus (P_2O_5)	0.6– 5.7	1.5
Potassium (K_2O)	0.3– 1.6	0.7

It is clear that digested sludges are not complete fertilizers and not balanced plant foods. They contain major and minor elements, growth promoting substances, and aid in replenishing organic matter, and as such are valuable as subgrade fertilizers and for soil improvement. Their value is not constant, but varies materially even at a given place.

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DISCUSSIONS

RÔLE OF THE LAND DURING FLOOD PERIODS

Discussion

BY L. K. SHERMAN, M. AM. SOC. C. E.

L. K. SHERMAN,[7] M. AM. SOC. C. E.[7a]—The subject of applied hydrology has been advanced appreciably by the publication of this paper. The writer has worked for several years along similar hydrologic lines relating to the measured effect of land, soil moisture, and vegetal cover on runoff and infiltration from natural rainfalls. His researches have been conducted independently of those made by Mr. Horner and on some basins differing materially in location and physical character from the basins described in the paper. The agreement between the two studies, as well as the lack of agreement, therefore, serves as a check or as a measure for the adoption of findings and procedures. The author's introduction gives a concise statement of the concepts and findings now generally approved in the science of hydrology.

Under "Infiltration Capacity" Mr. Horner defines the curve of infiltration capacity (sometimes called f-curve). This diagram is one of the most useful devices in the realm of hydrology. Rainfalls on the same basin or plot, occurring in the same month or season, will not give identical curves or parts of curves. However, these curves are generally in such agreement that they will furnish a composite f-curve which is sufficient for all practical purposes. A composite f-curve is more reliable than any curve from a single storm. The author assembles the curve at the point $f = 0.4$ to draw the average. The writer starts with a curve for a high value of f and successively moves the curve of next high f_o to the average point until the curve of lowest f_o is included. Tests made by the writer indicated that both methods furnish composites in close agreement. The important thing is to assemble all f-curves so as to maintain all of the highest f-values (f_o) at their original value on the f-scale. Vertical movement of position is not allowable. The writer finds that averaging f-curves by placing f_o at time zero, and then deriving average values at time intervals, is irrational and gives meaningless results. The same statement applies to the averaging of f_a-values.

NOTE.—This paper by W. W. Horner, M. Am. Soc. C. E., was published in May, 1943, *Proceedings*.

[7] Cons. Engr., Chicago, Ill.

[7a] Received by the Secretary May 11, 1943.

Many infiltration-capacity curves have been derived from infiltrometers. The ultimate minimum values of f_c on "wet" runs agree well with f_c-values from natural rainfall. The agreement on the f-curves is not very good. In the final analysis, field comparisons must be based on natural rainfalls.

Time condensation is mentioned by Mr. Horner, but its importance in the derivation of infiltration-capacity curves from natural rainfalls deserves re-emphasis. By definition, during a short time interval, the infiltration-capacity curve gives the infiltration volume at the "capacity rate." Frequently, in nature, the rain intensity may fall below the capacity rate. The rain during a certain hour may be only 0.25 in. per hr, whereas the infiltration capacity f at that hour may be 0.75 in. per hr. To meet the f-curve requirement of capacity, this hour of 0.25-in. rain must be shown on the f-curve as 20 min. The latter is condensed time—symbol T.

The procedures for deriving curves of infiltration capacity from natural rainfall include the following:

1. The Horner method, mentioned in the paper. Requires hydrographs of runoff closely synchronized with rain sequences or pattern. The method applies to plots and small basins.

2. The Sharp and Holtan method.[8] Requires hydrographs of runoff. Applies to small plots and large basins. First, the mass curve of infiltration is derived, and, from this, the f-curve is found.

3. The Sherman and Mayer method.[9] Requires the derivation of f_a (average infiltration capacity for the storm) and f_c. Requires only the total volume of surface runoff. The f-curve is found first, and the mass curve of infiltration is derived from it. Applies to small plots and large basins.

All methods require the sequence of rain intensities in time periods of one hour or less. All of the foregoing methods give f-curves that are in fair agreement.[10] (This applies to the Sharp and Holtan method after time condensation is applied. This was not done in their examples.[8])

The writer has compared Eq. 3 with the type of f-curves used in the Sherman and Mayer diagram. The term $\Delta F - f_c \, \Delta t$ represents the volume of infiltration under an f-curve and above the horizontal line of f_c. Eq. 3 therefore may be written:

$$\text{Volume above } f_c = V_f = \frac{f_{t1} - f_{t2}}{K} \qquad (4a)$$

In the Sherman and Mayer type of f-curve the volume above f_c between f_1 and f_c is:

$$V_f = (f_1 - f_c - 0.01)C_b \qquad (4b)$$

in which C_b is a constant. When Eq. 4b is applied to a segment of the f-curve between f_{t1} and f_{t2}, it becomes:

$$V_f = (f_{t1} - f_{t2})C_b \qquad (4c)$$

[8] "The Analysis of Hydrographs of Control-Plots and Small Homogeneous Water-Sheds," by A. L. Sharp and H. N. Holtan, *Transactions*, Am. Geophysical Union, Pt. II, 1942, p. 578.

[9] "Application of the Infiltration Theory to Engineering Practice," by L. K. Sherman and L. C. Mayer, *Transactions*, Am. Geophysical Union, Pt. III, 1941, p. 666.

[10] Paper by L. K. Sherman, presented at the meeting of the Section of Hydrology, Am. Geophysical Union, April, 1943.

Equating Eqs. 4*a* and 4*c*, the author's ratio $\frac{1}{K}$ is equal to C_b introduced by Messrs. Sherman and Mayer.

The Sherman and Mayer type of f-curve originally was derived graphically to conform to the requirement that, at any point on an f-curve, the hypotenuse of a triangle with $f - f_c$ as altitude and C_b (constant base) as base on the f_c-line was tangent to the curve at that point. The type of f-curve to fit the given data of f_a and f_c is selected by the equation:

$$C_b = \frac{f_a - f_c}{f_a} \qquad (5)$$

There appears to be a definite relation between the f-curve arcs in the paper and those introduced by Messrs. Sherman and Mayer. The composite f-curve is generally used in practical applications.

The author's M-curve and his stress upon the importance of considering antecedent soil moisture are of prime importance. Runoff from rainfall is meaningless if this factor of initial soil moisture is not specified.

Attention is directed to Table 6, containing data on brown loam soil in the basin of the Little Tallahatchie River of northeastern Mississippi.[11]

TABLE 6.—RUNOFF, IN INCHES, AT INITIAL SOIL MOISTURE; GROWING SEASON IN THE BASIN OF THE LITTLE TALLAHATCHIE RIVER

Cover	Rain	Very dry	Medium	Very wet
Bare	2.5	0.5	1.2	2.0
	3.5	0.8	1.9	2.6
Cultivated corn cotton	2.5	0.2	1.1	1.8
	3.5	0.4	1.7	2.4
Native grass	2.5	0.0	0.4	1.0
	3.5	0.0	0.7	1.4

The M-curve, as shown by Mr. Horner, is probably the most accurately devised scale or index of initial soil moisture yet presented. It may be susceptible of simplification, especially for purposes of comparing runoffs from different cover complexes on the same basin. The writer has used the following procedure with satisfactory results: Derive a mass curve of infiltration from the composite f-curve of the basin for a particular cover, season, or month. The base or time scale is the same as the composite f-curve. The volume of infiltration scale, or ordinates, in inches, is at the left. Zero of this scale corresponds to the point of tangency of the f-curve at f_c. The scale shows the capacity for absorbing infiltration; hence it is expressed in terms of soil moisture deficiency. The deficiency is 0 at f_c when the soil is very wet. The deficiency is greatest at an f_o for which the soil is very dry. The range on the mass scale between very wet and very dry runs from about -2.0 in. for bare soil, 3.0 in. for grass, and -4.0 or -5.0 in. for old forest cover. Points above zero, on the f_c-line are marked $+1.0$, $+2.0$, etc.

"Medium" initial soil moisture is halfway between 0 or very wet and the maximum or very dry. "Wet" and "dry" are the quarter points. "Medium" is used generally for cover comparisons of runoff. It has a different initial

[11] "Manual on Reduction of Runoff and Floods Through Agricultural Operations," by L. K. Sherman, 1942 (unpublished).

soil moisture for different covers. This is as it should be. Different vegetal covers evaporate and transpire different amounts of soil moisture for identical antecedent meteorological conditions. This scale was used in the foregoing tabulations of initial soil moisture and runoff. When the observed runoff is known, the numerical figure for initial soil moisture can be determined. This, as the author states, is simply the reverse case of computing runoff. With either scale, correlation with antecedent meteorological conditions (used in forecasting runoff) has not yet been accomplished.

In line with the author's findings on the Trinity River basin of Texas, the writer presents the following for the Little Tallahatchie River basin of Mississippi:

Description	Hudson Creek basin	Cane Creek basin
Drainage area, in miles	9.35	23.8
Soil	Brown loam	Pontotoc and Flatwoods
Initial soil moisture	Medium+14% toward dry	Medium
Date of observation	June 17, 1939	June 17, 1939
Basin f_c	0.17	0.11
Rainfall, in inches	1.34	3.11
Observed runoff, in inches	0.285	1.81
Computed runoff, in inches	0.26	1.86
Correction factor	1.09	0.97

A comparison of conditions, before and after certain assumed cover changes and gully controls, is as follows:

Description	Hudson Creek basin	Cane Creek basin
Medium Initial Soil Moisture Conditions:		
Rainfall, 2.70 In.—		
Runoff before assumed changes	1.09	1.46
Runoff after assumed changes	0.53	1.16
Rainfall, 4.35 In.—		
Runoff before assumed changes	2.40	2.52
Runoff after assumed changes	1.48	2.14

Some of the complexes used, like increased crop acreage, may increase the "after" runoff. The benefits under some plan designed by the U. S. Department of Agriculture may exceed this showing materially.

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DISCUSSIONS

EARTH PRESSURE AND SHEARING RESISTANCE OF PLASTIC CLAY
A SYMPOSIUM
Discussion

BY D. P. KRYNINE, M. AM. SOC. C. E.

D. P. KRYNINE,[37] M. AM. SOC. C. E.[37a]—Each of the four parts of this Symposium, namely (A) Foreword (nomenclature and notations), (B) "Liner-Plate Tunnels in the Chicago (Ill.) Subway," by Karl Terzaghi, (C) "Earth Pressure Measurements in Open Cuts, Chicago (Ill.) Subway," by Ralph B. Peck, and (D) "Earth Pressure on Tunnels," by W. S. Housel, respectively, will be discussed separately.

(A) *Nomenclature and Notations.*—According to the "Foreword," the symbol ϕ stands for the angle of internal shearing resistance of clay, ϕ_1 being the angle of shearing resistance in clay. The difference between these two terms is not clear. In Manual 22,[38] the term "angle of shearing resistance" does not appear at all. It follows that this term was introduced sometime in 1941 or 1942, and it would be helpful to have it explained. Professor Terzaghi uses the term in question, whereas, in interpreting the symbol ϕ, Professor Peck uses the expression "effective angle of internal friction of the clay" in presenting Eq. 6. Moreover, when dealing with the symbol in Eq. 13, Professor Peck defines it as the "angle of internal friction of the sand."

The writer has no intention of defending the term "angle of internal friction" as applied to plastic clays since he feels that, in this case, the term in question is incorrect; but he also believes that there is no justification for the new term "angle of shearing resistance" since the conception of the term "angle," which is rather tangible in the case of cohesionless and cohesive sands and similar materials, has nothing to do with the shearing resistance of plastic clays.

NOTE.—This Symposium was published in June, 1942, *Proceedings*. Discussion on this Symposium has appeared in *Proceedings*, as follows: September, 1942, by Messrs. Ralph H. Burke, L. G. Lenhardt, George E. Shafer, and M. E. Chamberlain; December, 1942, by A. A. Eremin, Assoc. M. Am. Soc. C. E.; April, 1943, by Messrs. A. E. Cummings, and D. M. Burmister; and May, 1943, by Gregory P. Tschebotarioff, M. Am. Soc. C. E.

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[37a] Received by the Secretary May 7, 1943.

[38] "Soil Mechanics Nomenclature," *Manual of Engineering Practice No. 22*, Am. Soc. C. E., 1941.

(*B*) *Paper by Professor Terzaghi.*—In the paper by Professor Terzaghi, formulas that control stability of the lining are given in Eqs. 1*b* and 2*b*, derived for the case of a blowout. In general, equations of the type of Eq. 1*b*, in which the left side represents the total bearing capacity of the structure and there is only one member on the right side, are unquestionably correct. This is not the case with equations of the type of Eq. 2*b*, in which the right side represents the sum of bearing capacities of two or more supporting agencies. In a general case, the bearing capacity of the entire structure may be less than the sum of the bearing capacities of the supporting agencies represented at the right side of the equation. To make this statement clear, assume a rigid beam on two supports (Fig. 51), the ultimate bearing capacity of the supports being 10 and 100 units, respectively. As the beam is gradually loaded, the entire structure possibly will fail when the load on the beam is more than 20 units—that is, far less than the value of 10 + 100 = 110 units. Both supports will fail simultaneously, however, if the increasing load is distributed between the supports in the ratio 10 : 100.

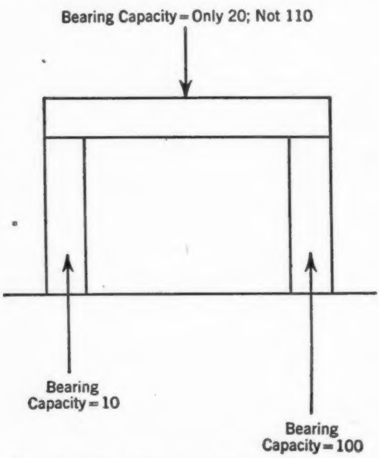

FIG. 51.—THE ULTIMATE BEARING CAPACITY OF A STRUCTURE MAY OR MAY NOT BE THE SUM OF THE ULTIMATE BEARING CAPACITIES OF ITS SUPPORTS

Before using equations of the type of Eq. 2*b*, their validity must be proved in each particular case. The lining of the Chicago subway was successful because the following system was satisfied:

$$W - S = R_1 + R_2 \qquad (38)$$

in which $R_1 \gtreqless 2\, q_r\, H_1$ and $R_2 \gtreqless A\, q_D$. Loads R_1 and R_2 are carried by the clay at the back of the tunnel and by the footings, respectively.

Clay Pressure on the Finished Tubes.—The difference between thick-walled and thin-walled concrete tubes is discussed by Professor Terzaghi under the heading, "Clay Pressure on Temporary and on Permanent Tunnel Support: Clay Pressure on the Finished Tubes"; and he makes the statement "that a thin concrete shell with a nominal reinforcement should suffice permanently for withstanding the clay pressure." The advantage offered by thin-walled tubes has been known for many years in the theory of pipe culverts. A deflected flexible pipe approaches the pressure line corresponding to the gradually increasing lateral pressure[39] just as shown in Fig. 4.

Soil Testing.—The statement made by Professor Terzaghi under the heading, "Soil Investigations: Soil Testing," really deserves the attention of the profession, especially of those engineers who are working on foundations and

[39] "Design of Pipe Culverts," by D. P. Krynine, *Proceedings*, Highway Research Board, 20th Annual Meeting, 1940.

earth structures. Slopes on soft, undisturbed clays fail if the average shearing stress on the surface of sliding becomes roughly equal to one half of the average nonconfined compressive strength of fairly undisturbed samples, regardless of the depth of the overburden. Commendably, Professor Terzaghi has applied this statement to tunnels and cuts. The procedure used in Chicago for determining the shearing resistance is simple and efficient, and can be duplicated at any project of importance without great effort and expense.

It should be noticed, however, that the statement advanced by Professor Terzaghi follows directly from applying Mohr's circle to the nonconfined compression test, for the case where the "angle of internal friction," $\phi = 0$. The writer also understands that, for many years, Professor Housel was of the opinion that the value of the so-called "angle of internal friction" in plastic clays was zero.

Danger-Spot Method.—It follows from Figs. 8 and 9 that at a depth of about 55 ft below the surface there is considerably stronger clay than at the actual location of the subway. It is not clear whether or not the problem of lowering the entire line has been studied in some detail. Such a lowering would have permitted the application of the liner-plate method in many places where actually the shield method was used. The disadvantages of this lowering are also obvious: Deeper stations would increase maintenance costs and the cost of some pipe installations; and the entrances to the tunnel perhaps would be shifted.

The writer still believes that the proper way to use Eq. 3 is to replace the values of $2\, q_r\, H_1$, and $A\, q_D$ by R_1 and R_2, respectively, from Eq. 38.

Experimental Section on Contract S-6.—This is interesting work, indeed, and Professor Terzaghi is to be commended for having done it so carefully. It is regrettable, however, that, since the abundant research material presented does not include pressures on the arch and data referring to the bending or buckling of vertical legs, a complete picture of the "deflected structure" cannot be established. Two circumstances, however, can be detected from Figs. 12 and 14: (1) All five positions of the "deflected structure" practically intersect at two points, so that the arch in Fig. 12 may be considered as having two hinges; and (2) the footings of the "deflected structure" were not horizontal but slightly inclined in the north-south direction; hence the bottoms of the legs were not strictly vertical. In fact, in the final position 9-4-41 (Fig. 14), the average reading of cells 17–19 was about 34% larger than that of cell 9; and the average reading of cells 16–18 was about 72% larger than the reading of cell 8. The northern footing of the experimental section was about one third overloaded in comparison with the southern footing and generally the bearing capacity of the clay was slightly exceeded ($2.67 > 2.5$ tons per sq ft). Although Professor Terzaghi states that, after a period of about 500 days, both points NL and SL became practically stationary, there is still a little jump in curve NL corresponding to about 535 days (Fig. 13).

The writer sincerely hopes, however, that, by placing a scale on his qualitative considerations referring to the experimental section, they will furnish negligible results.

Application of Test Results to the Design of Tunnel Tubes.—The writer agrees with Professor Terzaghi that the design shown in the right side of Fig. 15 is more economical and not less safe than that shown in the left side of Fig. 15. Precisely the same idea is followed in highway engineering when flexible pipes of corrugated metal are placed under embankments instead of heavy concrete pipes.

Professor Terzaghi's reasoning is quite correct for a single tube. In the case of thin double tubes, however, there may be a possibility of a rather considerable downward movement between the tubes (as explained subsequently under the heading, "Street Settlement") accompanied by the mutual

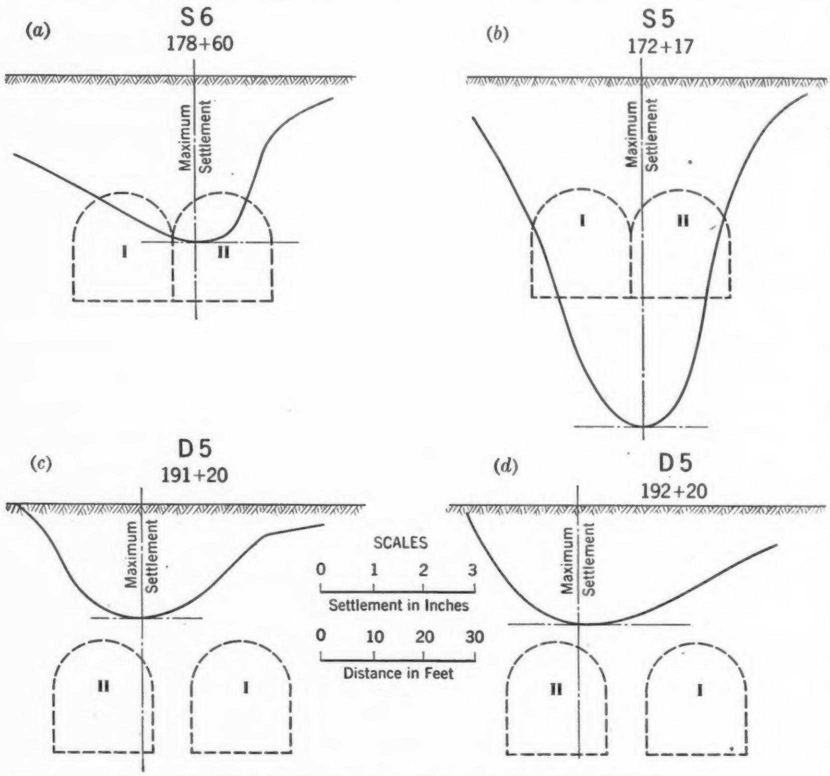

FIG. 52.—SUPERPOSITION OF CURVES I′ AND II′ IN FIG. 16.

leaning of the arches of the tubes toward each other. This may result in horizontal cracks at the outside wall of each tube tending to separate the arches from the inverts.

Street Settlement.—The writer superposed curves I′ and II′ in Fig. 16. The total settlement in all four cases shown in that figure is close to the center line of the tunnel with some eccentricity toward tube II (Fig. 52). Possibly, this means that, in all cases in question, the final vertical pressures on the earth close to the center line of the subway are larger than those under the tubes.

The best way to explain the difference in settlement between the twin tubes and the double tubes is to study the stress-strain relationship. The vertical pressures in the mass, starting from the earth surface and following the center line of the tunnel, are approximately the same in both cases. The character of the bodies on which these practically equal stresses act is approximately the same when tube I is mined but not the same when tube II is mined. In fact, the mass reinforced by the twin tubes strongly resists the action of the compression stress, whereas, in the case of double tubes, there is simple packing of the remolded earth into the space between the tubes, and hence there is larger settlement.

(*C*) *Paper by Professor Peck.*—The "Foreword" states that Parts II to VI of Professor Peck's paper are based on memoranda prepared by Professor Terzaghi. Nevertheless, from the first pages of Professor Peck's paper, the reader becomes aware of the author's initiative, preparedness, and energy, which in the final analysis resulted in submitting to the profession a considerable amount of useful information. In the writer's opinion, however, some of Professor Peck's material needs further clarification.

Strut Loads in Open Cut, Contract S-3.—In the "Synopsis" Professor Peck states that the distribution of the lateral pressure in open cuts was nonhydrostatic. At first glance, the table, Fig. 19(*e*), seems to offer proof of this statement. However, if a question is asked—when and why is the lateral pressure nonhydrostatic—the situation presents itself in a different light.

Assuming that, for relatively insignificant depths, the value of the "coefficient of pressure at rest" or "natural hydrostatic pressure ratio" K or K_0 is constant, the lateral pressure in a natural earth mass must be hydrostatic. Hence it was hydrostatic at the place where continuous sheet piling and H-piles were to be driven. During and after this driving, however, friction and shearing stresses developed in the mass in the same way as when a nail is driven into a wooden plank. When the excavation was being made, new shearing stresses developed in the mass because the earth mass tended to move toward the sheet piling which was being bent, and conservative nature opposed this disturbance by providing new shearing stresses. It is possible (but this remains to be proved) that all these systems of shearing stresses were self-balanced. In this case the lateral pressure on the sheet piling equaled the natural hydrostatic pressure; otherwise these two values would have been somewhat different. Because of the action of the shearing stresses, the pressure on the sheet piling was not hydrostatic, but redistributed (see data of November 20 in Fig. 19(*e*) and in Fig. 53).

Shearing stresses provided by nature to oppose movements within the mass may decrease or even completely vanish as time passes. This fact has been proved, for instance, by Professor Housel's observations in Detroit. The data in Fig. 19(*e*), referring to November 27, prove it also, quite conclusively. It seems that at the lower levels (El. 23.5 and El. 31.5), the lateral pressure tended to return to the natural hydrostatic shape, whereas the recovery at the intermediate layers (El. 14.5) was not so pronounced.

Several questions arise in studying Fig. 19(*e*): How much time is needed for complete recovery of a cut from shearing stresses, if it recovers at all?

If an excavation must stay braced for a long time, is it to be designed against both redistributed and hydrostatic pressure? In what cases, if any, does this phenomenon of recovery take place behind permanent flexible walls?

Rankine Earth Pressure Theory.—According to Professor Peck, Eqs. 6 and 7 were probably first derived by Résal (1910) as an extension of the Rankine

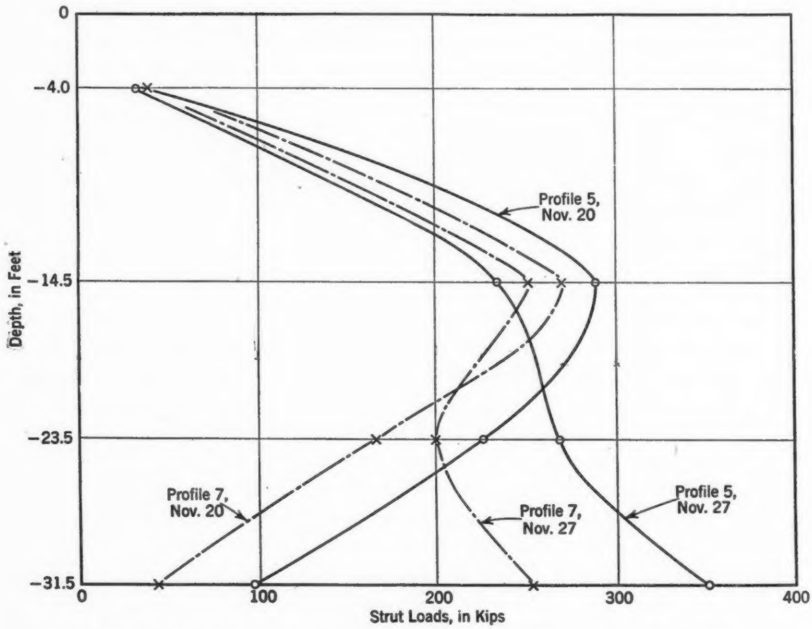

FIG. 53.—STRUT LOADS IN OPEN CUT, CONTRACT S-3 (DATA FROM FIG. 19(*e*))

theory. The writer has had the privilege of attending lectures by Résal, and he simply cannot admit that Résal did not know that Eq. 7 was published by Captain Français in 1820,[40] at which time Rankine was about one year old.

In writing Eq. 6 Professor Peck knew that, for his clay, $\phi = 0$. He should have made this statement and should have introduced this value into Eq. 6. This procedure would have simplified Eqs. 7, 8, 9, 11*a*, 11*b*, 15, 16*a*, 16*b*, 17, 18*a*, and 18*b*, and would have eliminated superfluous elementary mathematics. Thus the value of the paper would have been considerably increased. As an example, consider Eq. 18*b* if modified as the writer advises:

$$K_A + \frac{2\,q_a}{\gamma_a\,H} = 1 \qquad (39)$$

Obviously this is a straight-line relationship between K_A and $\frac{2\,q_a}{\gamma_a\,H}$, and Fig. 31 becomes quite unnecessary. Incidentally, Professor Peck is not entirely con-

[40] *Mémorial de l'officier du génie*, No. 4, 1820, p. 165, 4th line from the top.

sistent in his computations since on one occasion (namely, in writing Eq. 12) he did precisely what the writer advises and placed $\phi = 0$ in Eq. 6.

Apparently, Professor Peck shares a widespread opinion that the Rankine theory may be applied only to earth masses in the state of "plastic equilibrium" throughout, which is practically an impossibility. In fact Professor Peck states (see heading, "Part II.—Theory of Total Loads") that "In actual experience, opportunity for lateral expansion sufficient to develop a state of plastic equilibrium throughout the soil mass is never realized." To decide whether this opinion is correct, Rankine's writings need to be examined attentively. Rankine states:[41]

> "Theorem II. At each point of the mass of earth, the ratio of the difference of the greatest and least pressures to their sum cannot exceed the sine of the angle of repose."

Another symbolical expression of this theorem is as follows:

$$\frac{p_1}{p_2} \leqq \frac{1 + \sin \phi}{1 - \sin \phi} \qquad (40)$$

Rankine's symbols denote: p_1 = the greatest pressure (maximum principal stress); and p_2 = the least pressure (minimum principal stress). Rankine calls the angle ϕ "angle of repose." The tangent of the angle of repose, ϕ, according to Rankine,[42] equals the coefficient of friction. In other words, Rankine's "angle of repose" is what is now called "angle of friction."

The double sign in the Rankine formula (Eq. 40) clearly means that, according to Rankine, an earth mass may (sign $=$) or may not (sign $<$) be in a state of "plastic equilibrium."

In applying this theorem to a semi-infinite earth mass, Rankine made an incorrect statement; namely, that the lateral pressure, p_2, in such a mass is a minimum, which really means that the mass is in the state of plastic equilibrium. This statement is to be rejected; but theorem II is not affected by this rejection. It is still valid and may be applied to any mass, in the state of plastic equilibrium or otherwise.

General Wedge Theory for Earth Pressures.—The wedge that tends to separate from the backfill behind a wall or behind a brace is bounded by a curved surface. This was stated by Coulomb as early as 1773. Considerable work in determining the shape of this surface was done in Germany, starting, to the writer's knowledge, with Kötter (1888). Prandtl (1920) used the logarithmic spiral and so did Professor Terzaghi (1942). As is well known, Swedish engineers simply assumed that the sliding surface in the two-dimensional representation is an arc of a circle. After all, this is what Professor Peck does, but, instead of proceeding directly, he first uses the logarithmic spiral, places $\phi = 0$ in its equation, and thus indirectly arrives at an arc of a circle. The writer does not see why this should be done.

[41] "A Manual of Applied Mechanics," by W. J. M. Rankine, 15th Ed., London, 1898, pp. 213 and 214.
[42] *Ibid.*, p. 212.

Distribution of Pressure.—The writer agrees heartily with Professor Peck that (see heading, "Part IV.—Distribution of Pressure") " * * * it is useless to attempt to compute the real distribution of lateral pressure over the sheeting." In this case, as in the case of other structures, all that should be done is to design a structure that would be (*a*) safe and (*b*) economical. The statistical material contained in the paper by Professor Peck will be very helpful in reaching this objective.

(*D*) *Paper by Professor Housel.*—To the writer's knowledge, European literature contains no research data for the pressure on circular tunnels. The first work published (1937) along these lines was on the Lincoln Tunnel in New York.[43] Professor Housel's field work in Detroit began earlier, but the results were published later. More or less simultaneous research work concerning tunneling using the shield method has been done in Chicago.[2a]

The paper by Professor Housel contains so many interesting and important features that, to discuss it, it would be necessary to write another paper. The writer wishes only to emphasize some items of particular significance.

Duration of Observations.—Fig. 40 shows some observations that covered the period from December, 1930, to January, 1941—about eleven years. No one has previously observed the pressure on a tunnel lining for that long a period; and this circumstance permitted Professor Housel to draw some important conclusions.

Variation of Pressure with Time.—Fig. 41 is an interesting diagram showing the recovery of an earth mass disturbed by the tunneling. It seems that the mass tends to return to its original state of stress which is analogous to the healing capacity of a living organism. However, just as scars and spots remain on the skin after the healing of a wound, so there may be permanent or residual shearing stresses in the mass as the result of the disturbance of its internal equilibrium.

Hypothesis of Shearing Stresses in the Mass.—It follows from the conditions of equilibrium of a small cube within an earth mass that the weight of this cube is balanced by both the vertical pressure and the vertical shearing stress. Thus the vertical pressure at a point of a mass is not the same as the weight of the mass above that point. If the shearing stress is increased because of some disturbance in the mass such as tunneling, the vertical pressure will decrease accordingly. This explains the phenomenon sometimes termed "arching" or the sidewise transfer of weight of the mass over the tunnel. Evidently, the graph of "redistribution of vertical pressure" as shown in Fig. 42 is but a very crude approximation. A vast field is open to the research workers in the province of these shearing stresses: Their physical nature should be investigated and the laws controlling their increase and decrease formulated. Nowhere, however, has the problem of the action of shearing stresses over a tunnel been approached in such detail as in Professor Housel's paper, and he is to be highly commended for this achievement.

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[43] "Lincoln Tunnel: The Field Measurements and Study of Stresses in the Tunnel Lining," by G. M. Rapp and A. H. Baker, Port of New York Authority, July, 1937 (lithoprinted).

[2a] "Shield Tunnels of the Chicago Subway," by Karl Terzaghi, *Journal*, Boston Soc. of Civ. Engrs., Vol. 29, July, 1942.

Conclusion.—The cities of Chicago and Detroit are to be complimented for organizing soil investigations and research measurements during the tunnel building in those cities. This is precisely what large cities and government organizations should do when undertaking considerable building projects. Both cities are also to be congratulated for the able choice of their research workers who produced a mass of useful information contained in this Symposium.

Corrections for *Transactions:* In June, 1942, *Proceedings*, page 886, line 7, change "0.85" to "0.58"; page 901, ordinates in Fig. 19(*d*) should read 0, 5, 10, etc.; page 917, in Fig. 33 change "$\frac{H}{H_c}$" to "$\frac{H\gamma}{2q}$"; and, on page 950, line 7, change "$>9{,}120$" to "$<9{,}120$."

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

THE HYDRAULIC JUMP IN SLOPING CHANNELS

Discussion

By Jerome Fee, Assoc. M. Am. Soc. C. E.

Jerome Fee,[24] Assoc. M. Am. Soc. C. E.[24a]—Few investigations of any kind have been made of flow in open channels with steep slopes. Therefore, the author has performed a valuable service to the engineering profession by his study of the hydraulic jump in sloping channels. By further experiments the method easily can be extended to cover a wide range of conditions.

Fig. 6, showing the relation between the kinetic flow factor and several essential elements of the hydraulic jump, is particularly interesting. The pressure-coefficient curve is defined with surprising accuracy; and even the length of roller is more amenable to measurement than might be expected.

In view of the greater accuracy of the pressure-coefficient curve, it would appear desirable to obtain similar curves for other slopes by direct piezometric measurements, as was done in this case, rather than to derive values of the pressure coefficient from Eq. 18, which depends directly on the length of roller.

Although the momentum law is used instead of energy considerations to determine the dimensions of the hydraulic jump, the total loss of energy under various conditions is one of the most important features of this problem, particularly where the jump is used as a means of dissipating energy. The author's method can be applied directly to the determination of energy loss, and thus it will be possible to compare the efficiency, as energy dissipators, of the four classes of hydraulic jump described in this paper.

The writer has been interested particularly in the energy phase of flow in steeply sloping channels. The energy line in such cases is not the simple line with which the engineer is familiar under ordinary flat slope conditions. To borrow a term from modern physics, the energy line has a "fine structure" analogous to certain spectroscopic lines which were believed originally to be single lines and later each was found to consist of several lines, very close together, which could be separated under certain circumstances.

Note.—This paper by Carl E. Kindsvater, Jun. Am. Soc. C. E., was published in November, 1942, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: February, 1943, by Messrs. Joe W. Johnson, and Karl R. Kennison; and May, 1943, by Messrs. J. C. Stevens, and C. J. Posey.

[24] Engr., Federal Power Commission, San Francisco, Calif.

[24a] Received by the Secretary April 16, 1943.

This may be shown by the construction of Fig. 17. In Section A–A, normal to the bottom, particles indicated by the letters m, n, o, and p are all at the same energy level, Z–Z. For example, particle o has a pressure head equal to o–o′ and a velocity head equal to o′–o″, as determined by the velocity in the direction of flow, parallel to the slope. From an energy standpoint, particle o is in exactly the same condition as it would be at point o″. The

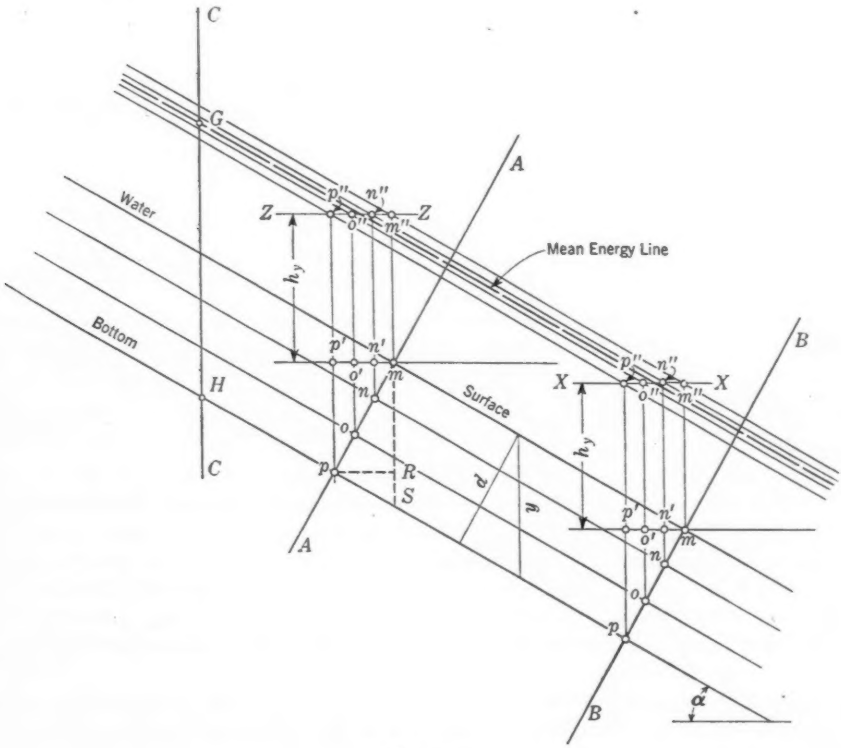

Fig. 17

same argument applies to Section B–B, also normal to the slope. Particle o at this section, of course, is not the same particle o as at Section A–A but a different particle at the same distance from the bottom. This particle is at energy level, X–X, being represented by point o″ on the horizontal line, X–X.

The sloping line o″–o″, which for uniform flow is parallel to the water surface, is the energy line for all particles at the same distance above the bottom as particle o. Similarly, lines m″–m″, n″–n″, etc., are energy lines for particles at respective depths corresponding to points m, n, etc. Instead of a single energy line, there is a band of energy lines between m″–m″ and p″–p″.

The significance of these lines is apparent when one considers a vertical section, C–C. The point of intersection of line o″–o″ and Section C–C gives the energy level of the particle defined by the intersection of line o–o and C–C.

Vertical sections, such as C–C, are the only practical means of defining the position of particles flowing in a conduit, and, to be of any value, energy lines must be correlated with vertical sections in the manner shown.

The average energy level of all particles in a vertical section is at the point of intersection of the "mean energy line" (shown in Fig. 17) with the section itself. The vertical distance from the bottom of the conduit to the mean energy line is readily determined as

$$\overline{HG} = y + \beta \frac{V^2}{2\,g} - \frac{d}{2} \sin\alpha \tan\alpha \ldots\ldots\ldots\ldots\ldots\ldots (53)$$

in which β is a constant greater than unity, introduced by inequalities in velocity across the section. The term $\frac{d}{2} \sin\alpha \tan\alpha$ is equal to one half of the distance RS, in Fig. 17.

In the foregoing analysis it is assumed that the reader is acquainted with the formula:

$$p = \gamma\, d \cos\alpha \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (54)$$

The author refers to the derivation of this equation[7] by Messrs. O'Brien and Hickox, who, in turn, ascribe the equation to Harald Lauffer. There is apparently some discrepancy in the text cited, since it contains the following erroneous equation:

$$(y - z) \cos\theta + z = y \cos\theta \ldots\ldots\ldots\ldots\ldots\ldots (55)$$

Eq. 55 leads to $z = z \cos\theta$, which is clearly incorrect.

The writer has not had an opportunity of learning the date and reference of Lauffer's work, which would doubtless be of interest to others. Possibly the author can supply this information.

The late John Hedberg,[25] Assoc. M. Am. Soc. C. E., presented an excellent proof of the equation, $p = \gamma\, d \cos\alpha$. Although he was evidently not the first, Mr. Hedberg gave a derivation that hardly could be improved upon—certainly not in point of brevity.

In developing Eq. 8 the author refers to "the equation of equilibrium for section 1" of Fig. 5(*b*). It would seem that Eq. 8 depends fundamentally on the equilibrium of the body of water between two sections; namely, section 1 and a section normal to the slope which intersects section 1 at the water surface. One section, alone, is not enough in this case to be the basis of an equilibrium equation.

It is a pleasure to find such a valuable contribution to the study of flow on steep slopes. The author is to be congratulated for successfully carrying out an investigation presenting unusual difficulties, both experimental and analytical.

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[7] "Applied Fluid Mechanics," by Morrough P. O'Brien and George H. Hickox, McGraw-Hill Book Co., Inc., New York, N. Y., p. 293.

[25] "Flow on Steep Slopes," by John Hedberg, *Civil Engineering*, September, 1937, p. 633.

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

APPLICATION OF SOIL MECHANICS IN DESIGNING BUILDING FOUNDATIONS

Discussion

BY MESSRS. GREGORY P. TSCHEBOTARIOFF, GUTHLAC WILSON, W. H. McALPINE, AND KARL TERZAGHI

GREGORY P. TSCHEBOTARIOFF,[22] M. AM. SOC. C. E.[22a]—Two successful foundation designs, based on the principles of soil mechanics, are brought to the attention of the engineering profession by the authors. The time-settlement records of the two buildings, designed by the authors, as compared with the records of three other older buildings founded on similar soil strata at Boston, clearly indicate the advantages of the "floating" type of foundation. Both the maximum and the differential settlements of these two new buildings are much smaller than the ones for buildings with foundations designed in the conventional manner. For a long time Professor Casagrande has been an ardent advocate of the "floating" type of foundations on clays. The behavior of these two buildings, therefore, should give him justifiable cause for satisfaction.

The design of the latter of the two appeals more to the writer. The use of basement walls as stiffening foundation girders provides for a much greater rigidity of the entire structure as compared with the cantilever system used in the first building. This should be of considerable importance in other localities where new designs might have to be made for soil deposits about which fewer data are available than in Boston. The greater general rigidity and the equalizing action of the deep-wall girders should prove very helpful in all cases where the compressibility of a deposit is not entirely uniform. In both buildings, an attempt is made to further equalize the "dishing" effect of deep-seated settlements by increasing the loading at the periphery, and especially at the corners. In the second building, this is done in a very simple manner by decreasing the width of the footings at the periphery and at the corners. The

NOTE.—This paper by A. Casagrande, Assoc. M. Am. Soc. C. E., and R. E. Fadum, Jun. Am. Soc. C. E., was published in November, 1942, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: March, 1943, by Messrs. Jacob Feld, and Leonard Zeevaert.

[22] Asst. Prof., Civ. Eng., Princeton Univ., Princeton, N. J.

[22a] Received by the Secretary April 8, 1943.

resulting likely variation in the shallow-seated settlements should help in compensating the usual tendency for a greater settlement of the center of a foundation supported by a deep clay stratum. This arrangement, in combination with the use of basement girder walls for load distribution purposes, appears to the writer as being much more economical and effective than the cantilever loading system adopted in the first building with a similar aim. This second arrangement also should be more helpful in counteracting possible harmful effects of any deeper future excavations (for instance, subways) in the vicinity of structures of this type.

The writer heartily agrees with the general Conclusions (1), (2), (3), (5), and (6). However, insufficient evidence is yet available to justify Conclusion (4) that "The settlements due to deformation and recompression following swelling take place essentially during the construction period. * * *" This statement appears to be true under the loading and soil conditions described by the authors, but it does not follow necessarily that it will hold under different conditions. An examination of Fig. 22 shows that the heaving of the bottom of the excavation pit continued without interruption, and even at an increased rate, after the excavation was completed. Only the installation of the foundation slab stopped and decreased the heaving. In the data presented, there is no evidence to show that the heaving would not have continued for an appreciable time had the excavation pit been left open for a longer period. It is reasonable to assume that the heaving of the bottom of the excavation pit is due to two causes: (1) A deformation involving the squeezing of soil into the pit without change of density; and (2) a swelling of the clay as a result of the removal of the overburden. The first cause would predominate in very soft clays, and the greater part of the action should be of relatively short duration. The second cause would be of greater importance in stiffer materials and might last longer. It would appear that, in the case of the Boston buildings, no appreciable swelling could have occurred because of the relatively short period of excavation.

The time element in this kind of work is likely to prove of considerable importance. Gradual absorption of water causes the clay to swell. If this were permitted to occur through a stratum of considerable depth, it is only natural to assume that the settlements following recompression might last for a correspondingly long period and would be much larger. It is not customary to leave excavation pits open for a long time, but it has happened. The writer believes, therefore, that the likely importance of the time element should be emphasized.

Practically no information is contained in the paper concerning the physical characteristics of the clay underlying the two buildings. It is to be hoped that, in their closing discussion, the authors will give this supplementary information. Such descriptions as "hard" or "soft" clay generally are recognized as insufficient to describe a soil properly and in a manner permitting other engineers to visualize its nature.[23] In the paper, the only data concerning the type of clay supporting the buildings described, is contained in

[23] "Exploration of Soil Conditions and Sampling Operations," by H. A. Mohr, *Soil Mechanics Series No. 9*, Harvard Univ., February, 1940, 2d Ed., p. 5.

Fig. 15, with the clay defined only as "hard" or "stiff" and "soft." No water contents, either in the form of average or limit values, nor any other numerical values of properties important for the visualization and classification of the soil are given. The stratified nature of the clay was mentioned. Therefore, both average values and values related to the coarser and finer thin strata would be of interest. Without such data, the paper will be incomplete, as it will not permit engineers in other localities to relate the observations on their soils to those reported by the authors. It is to be hoped, therefore, that these data will be given in the closing discussion.

The general discussion of the basic soil mechanics principles underlying the design of foundations is presented in a clear and concise manner. There is one point of secondary importance, however, with which the writer does not agree entirely, and which should be stressed since considerable misconceptions have existed on the matter in the past. The authors (see heading, "Soil Properties Responsible for Settlements: Stress-Deformation Characteristics of Cohesionless Soils (*g*)") state that the "concentration factor" is no improvement over the theory of elasticity. The reason offered is that when an integration over a finite area is made (using the concentration factor) then essentially the same type of stress distribution results as that shown in Fig. 4.

The writer fully agrees with the conclusion that the concentration factor, as used so far, does not present any actual improvement. However, the very attempt to integrate the concentration factor over a finite area indicates a misconception concerning the physical conditions responsible for the variation of the soil reactions under foundations underlain by sand, as compared with those underlain by clay. In this connection, the writer wishes to point out that the deflection curve of a large foundation resting upon sand, as shown in Fig. 5, does not appear to be correct. Much more likely is the form of the deflection curve shown in Fig. 25(*b*), the idea suggested by the authors being as shown in Fig. 25(*a*).

FIG. 25.—NATURE OF DEFLECTION (*a*) OF A LARGE UNIFORMLY LOADED FLEXIBLE FOUNDATION MAT RESTING ON SAND, AND (*b*) THE DISTRIBUTION OF SOIL REACTIONS

A zero value of the soil reaction at the edge of a footing underlain by sand is largely due to the fact that the sand can have a shearing resistance only in the presence of a normal stress mobilizing the friction between grains. Such a stress is present under the central part of a footing, but is nonexistent at the edge. In other words, this decrease in the value of the soil reaction is essentially a boundary condition. An attempt to express this boundary condition by the concentration factor can lead to results in agreement with known facts only if the concentration factor is assumed as varying with the distance from

the edge of a footing. An integration with a constant value of a concentration factor over the entire area naturally leads to unreasonable results, since the basic assumption underlying such action has no foundation in physical facts.

A large area is likely to deflect in the manner shown by Fig. 25(*b*), since at a certain depth the pressures on a horizontal layer of any soil will be greater beneath the center of a foundation than at its edges. In other words, the effect of the boundary surface condition will decrease with depth, and, for a large loaded area, will be noticeable only at the edges; whereas, beneath the central part of the foundation, the stress distribution in sand should not vary greatly from that of other materials. The point is only of theoretical interest since settlements on sand of normal density are very small in any case.

In conclusion, the writer wishes to emphasize that his criticisms are only of secondary importance, and that the authors are to be complimented on their successful designs.

GUTHLAC WILSON,[24] M. AM. SOC. C. E.[24a]—All engineers concerned with the design of building foundations will find much valuable material in this paper. The writer is particularly interested in the excellent descriptions of two floating or ship-like foundations, a type of foundation that was first brought to his attention by S. E. Faber, M. Am. Soc. C. E., in the course of discussions in Shanghai, China, in the 1930's. It is most interesting to note that this type of foundation is economically superior to the piled foundation even where, as in Boston, a stratum of hardpan is within easy reach of piles.

However, although in a strict sense the authors are correct, they seem to have been somewhat too sweeping in their statement (see heading, "Examples of the Application of Soil Mechanics to the Design of Building Foundations: The New England Mutual Life Insurance Company Building—Observation of Movements"):

> "* * * Were it possible to distribute the building load uniformly over the building area, and were it possible to excavate and apply the building load simultaneously, no settlement would result. However, it is impracticable to design and construct a building in such a manner that these requirements are fulfilled, * * *."

It appears to the writer that these requirements could be more nearly satisfied, and would be, were it justifiable economically. Assume that a building similar to those described were to be built in another location and that there were no layer of hard or stiff clay overlying the soft clay, as in Boston. The engineer's object would then be to change an overburden of 2 tons per sq ft of organic silt clay and fill at the foundation depth of 40 ft into a uniform foundation pressure of 2 tons per sq ft with the minimum of disturbance. This change could be best accomplished by an adaptation of "The Chicago Method." The outer basement walls and the interior main walls, each with a sufficient area of footing, could be constructed in trenches; the ground floor and a suitable amount of the superstructure could then be built upon these; the base-

[24] Director, Constructional Design, Ministry of Works, London, England.

[24a] Received by the Secretary April 8, 1943.

ment volume would later be excavated by sections between interior main walls —perhaps each section would be excavated in a series of parallel slices; and the first basement floor and the main foundation floor could be constructed. As only a small area of the foundation would be opened up at one time, the vertical swelling, or "negative settlement" would be minimized. In extreme cases, it might be worth while to resort to compressed air. The main basement floor could probably be built economically as a series of parallel and continuous barrel-vault arches, designed on the lines of the well-known Zeiss-Dewidag roof. Such an arch of 60-ft span by 30-ft width would bear approximately 2 tons per sq ft if it were 1.5 ft thick. A suggested foundation along these lines is illustrated by Fig. 26.

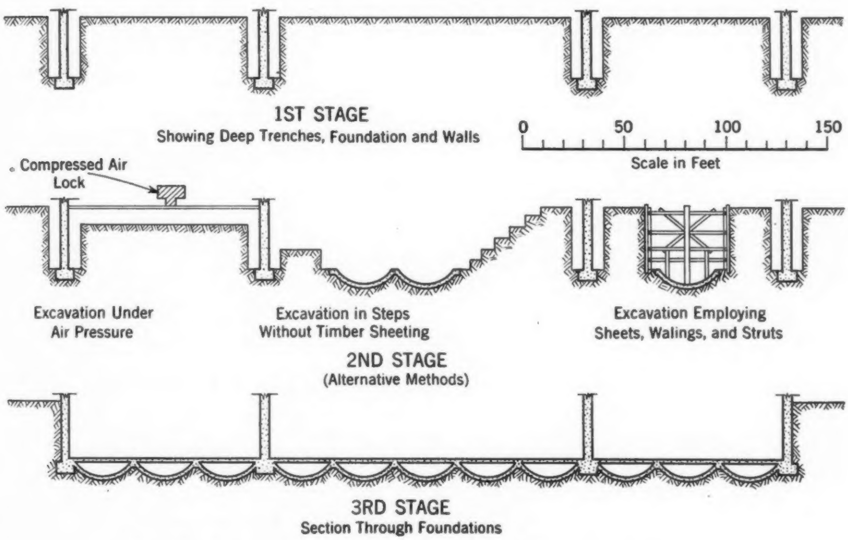

FIG. 26.—SUGGESTED METHOD OF CONSTRUCTING FLOATING FOUNDATION

There is so much information packed into the paper that the writer rather hesitates to ask for more. However, he hopes that the authors will enrich their paper by the addition of information stating what live loads were considered in the foundation computations, what reduction factors, if any, were used, and whether these reduction factors were the same as those used in the design of the columns. Also, they should add diagrams showing the story heights of various parts of the New England Mutual and Liberty Mutual buildings.

W. H. McALPINE,[25] M. AM. Soc. C. E.[25a]—The analysis of the design of foundation conditions for the New England Mutual Life Insurance Company Building and the results obtained in the completed structure are of special

[25] Chf. Engr., Office, Chf. of Engrs., U. S. Army, Washington, D. C.

[25a] Received by the Secretary April 20, 1943.

interest to the writer, perhaps in part because the new structure replaces old structures reminiscent of his school days long ago.

It is noted that, as a result of the settlement investigations, it was decided to use the principle of a floating foundation rather than to carry the load on piles driven through the soft clay stratum into hardpan 125 ft below the ground surface, preliminary plans indicating the former to be the cheaper plan. The decision to adopt the floating foundation plan, in spite of the fact that the surrounding buildings were on piles, must have required considerable courage and a thorough knowledge and confidence in the results of laboratory tests of the soils. Apparently the work in all its steps was planned carefully.

A study of the settlement records appears to justify the foundation plan adopted. The paper emphasizes the importance of both a thorough examination of the subsoils by deep borings and soil tests of the sampled material in connection with the design of foundations for important structures where there is any likelihood of encountering soft material. Too frequently, the mistake is made of being satisfied with shallow borings that fail to disclose soft clay layers at greater depth which often result in serious settlement of the structure. The foundation design of important structures should be based on a thorough investigation of the substrata, taking into consideration the permissible settlement that can take place without impairing the structure. In some cases, considerable inequalities in settlement can be tolerated, whereas, in a gun block for large coastal batteries, any inequality in settlement would be serious. In some instances, taking relative economy into consideration, it may be a close decision as to whether to use piles to carry the load to firm material or to adopt the floating type of foundation.

Referring to Fig. 23, an explanation as to the reason for the greater settlement on the Newbury Street side than on the Boylston Street side would be desirable. Also, a statement as to whether any of the adjacent streets or buildings were adversely affected during or after the construction period would be of interest.

The "Conclusions" of the authors appear sound on the basis of the facts presented.

KARL TERZAGHI,[26] M. AM. SOC. C. E.[26a]—A description of the foundations of two large buildings in Boston, Mass., forms the basis of this stimulating paper. The success of these foundations is due primarily to the fact that the weight of the structures is roughly equal to the weight of the soil removed before the application of the weight of the buildings.

The principle of this simple method for avoiding important settlement of buildings on soft ground has been known for more than a century. It is described in a classical handbook by G. Hagen whose third edition appeared in 1870.[27] After outlining the general procedure, Hagen states that the total weight of the structure should not be greater than the weight of the mass of mud which previously occupied the site of the building. As an example of the

[26] Winchester, Mass.

[26a] Received by the Secretary April 26, 1943.

[27] "Handbuch der Wasserbaukunst," by G. Hagen, Berlin, Ernst & Korn, 1st Pt., Vol. 2, 3d Ed., 1870.

successful application of the method he mentions the foundation of the Albion mills in London, England, by Rennie (1761–1821).

In the design of the foundations described in the paper advantage has been taken of the stiffness of the top crust of the Boston blue clay. This procedure was also recommended by Hagen, who quotes as an example the foundation of a ropery in Rochefort in France, designed by Blondel, early in the nineteenth century. The structure rests on a 12-ft layer of stiff mud which constitutes the top layer of a very deep stratum of soft mud.

These examples demonstrate that the most important method at the disposal of present-day engineers, for reducing or eliminating the settlement of structures, is very old. Therefore, many readers of the paper may feel tempted to inquire exactly what contribution to soil mechanics is included in it. To find an answer to this question one may turn to the more recent handbooks—for instance, the German handbook on foundations,[28] published in 1906, which constitutes the successor to the older writings. This more recent and more elaborate treatise does not even mention the method for eliminating the settlement which was well known to the older generation of engineers. The reason is quite obvious: The designs of Rennie and of Blondel originated in flashes of engineering intuition, and the capacity for intuition is not hereditary. Methods of design based on intuition cannot be, and are not, practiced by the average engineer until they become an integral part of a logical system which demonstrates their usefulness on the basis of theoretical principles. Soil mechanics has established such a system.

Still more important is the fact that soil mechanics succeeded in establishing methods for estimating the settlement of structures in advance of construction. Some of the theories on which the computations are based originated late in the nineteenth century. Nevertheless in none of the handbooks on foundations published before 1920 did settlement receive more than casual attention. The consequence of this prevalent ignorance concerning settlement is stored away in the records of countless lawsuits, and they are demonstrated by the curves III to V in Fig. 15. The method for preventing important differential settlements, such as those shown by the curves III to V, was already known prior to 1870, but the designers had no inducement to use it, because they were unable to estimate in advance the settlement of the foundation which they designed. None of the important and spectacular settlements that have come to the writer's attention have been anticipated. They came as "Acts of God." This fact makes the issue clear enough. Soil mechanics did not grow out of the need for novel methods of foundations. It grew out of the urgent need for reliable rules for using the existing ones with less risk. Once the knowledge of the factors that determine the settlement of foundations becomes commonplace, the frequency of inadequate foundations and of surprise settlements will diminish automatically. In other words, in the future, the average engineer will be capable of accomplishing that which, in the past, has been the prerogative of those very few who are gifted with engineering intuition. The

[28] "Handbuch der Ingenieurwissenschaften," III. Band, Der Grundbau, 4th Ed., 1906, W. Engelmann, Leipzig.

average person does not see except that which he has been taught to look for and soil mechanics serves the function of a mentor.

In the paper the description of the two Boston foundations is preceded by a set of general statements concerning the factors that determine the settlement of structures. The writer believes that many of these statements require some qualification.

Statements (*a*) to (*m*), under the heading "Introduction: Stress-Deformation Characteristics of Cohesionless Soils," deal with cohesionless soils. According to the first sentence of statement (*a*), the contact pressure on a rigid area resting on the surface of a cohesionless soil increases from zero at the edge to a maximum at the center. This statement is confirmed by both theory and experiment. However, in practice, the base of every footing is at a certain depth h below the surface. If γ is the unit weight of the sand and ϕ the angle of internal friction, the unit pressure at the edge of the footing can be as great as

$$q_o = \gamma\, h \tan^4 \left(45^\circ + \frac{\phi}{2}\right) \qquad (2)$$

Therefore the pressure at the edge of a footing is by no means necessarily equal to zero. As a matter of fact, the writer has never seen a record obtained from pressure-cell observations on actual footings which would justify the assumption that the contact pressure along the edge of a footing is equal to zero.

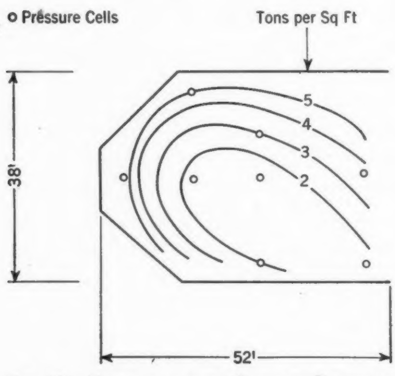

FIG. 27.—DISTRIBUTION OF CONTACT PRESSURE ON THE BASE OF A CONCRETE BRIDGE PIER ON SAND, AT A DEPTH OF 34 FT BELOW THE RIVER BOTTOM

Statement (*b*), illustrated by Fig. 3(*c*), is at variance with the only one set of observations concerning the distribution of the contact pressure on the base of a pier that has come to the writer's attention. Fig. 27 shows the base of the pier, at a depth of about 34 ft below the bottom of the Rhine River. It rests on a stratum of dense, coarse sand with fine gravel. The pier was constructed by the compressed air method. Using the notation indicated in Fig. 3, the value $p = \frac{P}{A}$ was about 4 tons per sq ft and the value $(b + h)\,\gamma$ about 1.8 tons per sq ft. The contact pressure was measured by means of eight pressure cells of the modified Goldbeck type.[29] The lines of equal pressure indicate that the contact pressure increased very considerably from the central part of the base toward the edges. If Fig. 3(*c*) is based on sets of field measurements unknown to the writer, a presentation of the factual data would help in clarifying the issue.

[29] "Der Bau der neuen Rheinbrücke bei Ludwigshafen-Mannheim," by R. Burger, *Die Bautechnik*, Vol. 10, 1932.

According to statement (*c*) the ultimate bearing capacity of a cohesionless soil increases approximately in direct proportion to the width of the loaded area. This statement is valid only if the coefficient of internal friction (ϕ) of the sand is almost independent of the pressure. Since the ultimate bearing capacity increases approximately in direct proportion to the fourth power of $\tan\left(45° + \frac{\phi}{2}\right)$, a small decrease of ϕ causes an important deviation from the rule which is based on the assumption that ϕ is constant. Therefore, the rule does not apply to sands whose angle of internal friction decreases appreciably with increasing pressure.

The validity of the rule expressed by statement (*c*) has been contested repeatedly, the last time by B. K. Hough, Jr.,[30] Assoc. M. Am. Soc. C. E. According to Mr. Hough, the rule is not in accordance with the results of large-scale field tests. The preceding paragraph leaves no doubt that, in many instances, properly conducted large-scale field tests would disclose important departures from the rule expressed by statement (*c*); yet the writer has not yet seen any reliable record that would either confirm or contradict the statement. The reason is consistently the same: The small footings are loaded to failure whereas the test on the larger ones is discontinued at an earlier stage. The elastic properties of sand are such that the soil support of a footing on sand does not really fail until the settlement exceeds a certain percentage, $n\%$ of the width of the loaded area, regardless of what this width may be. For dense sand the value $n\%$ is about five and for loose sand ten or fifteen. Extrapolation from the load corresponding to a smaller settlement to the failure load cannot be relied upon. In this connection it should be noted that the unit load required to produce a given settlement or a conspicuous increase of the slope of the load-settlement curve has nothing in common with the ultimate bearing capacity referred to in statement (*c*).

Statement (*d*) implies that the ultimate bearing capacity of a loaded area increases with the square of the depth. This statement seems to be open to objections. In the 1920's the writer developed an approximate theory for computing the ultimate bearing capacity of shallow foundations such as circular footings.[31] According to this theory the ultimate bearing capacity q_h of a circular footing with a radius R which rests at a depth h below the surface on a mass of dry sand with an angle of internal friction 36° is roughly equal to

$$q_h = q_o \left[1 + \frac{h}{R} + 0.146 \left(\frac{h}{R}\right)^2\right] \ldots\ldots\ldots\ldots\ldots\ldots (3)$$

in which q_o is the ultimate bearing capacity of the same footing for $h = 0$. Fig. 28(*a*) is a vertical section through the footing. The vertical pressure that acts on the sand within zone I causes the sand to spread in radial, horizontal directions. The sand within zone II tends to rise, because it is laterally confined. The rise is resisted by the weight W of the sand within zone III, by the skin friction along the vertical surface of the footing and by the full shearing

[30] Discussion by B. K. Hough, Jr., of the paper, "Relation of Undisturbed Sampling to Laboratory Testing," by P. C. Rutledge, *Proceedings*, Am. Soc. C. E., December, 1942, pp. 1833–1836.

[31] "Erdbaumechanik auf bodenphysikalischer Grundlage," by Karl Terzaghi, Leipzig, Deuticke, 1925.

resistance of the sand along the outer, cylindrical boundary ef of zone III. This shearing resistance is represented by the shaded area efg in Fig. 28(*a*).

In Fig. 28(*b*), Eq. 3 is represented by the curve N_1. For values of $\frac{h}{R}$ between 1 and 4 the curve is almost straight and it can be represented almost exactly by the simple equation

$$q_h = 1.8 \left(\frac{h}{R}\right) q_o \qquad \left(1 > \frac{h}{R} > 4\right) \ldots\ldots\ldots\ldots\ldots (4)$$

Eq. 4 covers the range of $\frac{h}{R}$ for circular footings. On the other hand, if $\frac{h}{R}$ is greater than 4, Eq. 3 is not even approximately valid. This statement is illustrated by Fig. 28(*c*) which represents a vertical section through a cylindrical

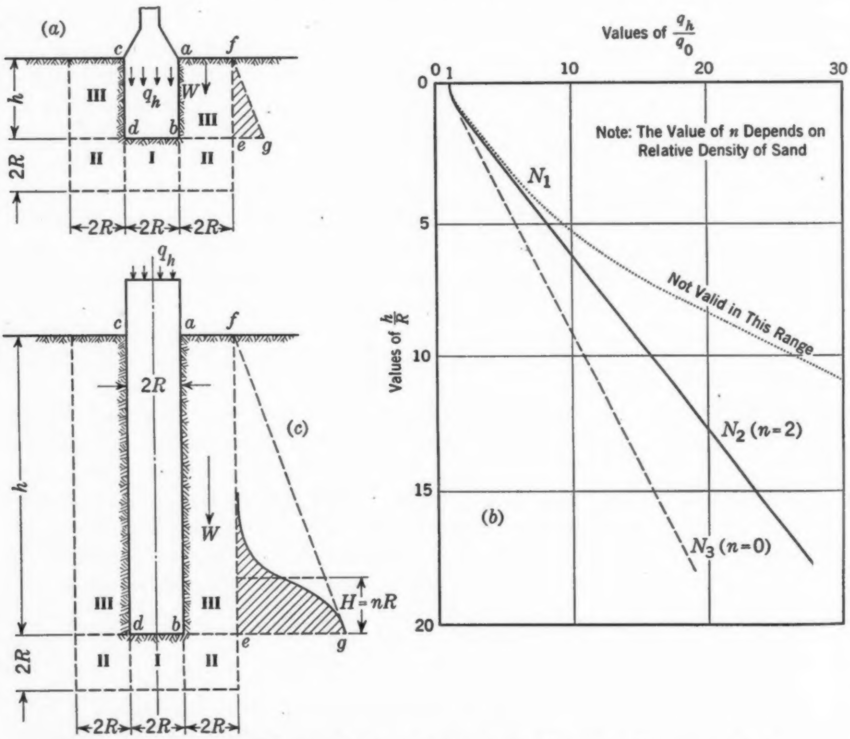

FIG. 28.—RELATION BETWEEN BEARING CAPACITY AND DEPTH OF FOUNDATION FOR CIRCULAR FOOTINGS (*a*) AND CYLINDRICAL PIERS (*c*).

pier. Since the height of zone III is great compared to its width, the shearing stresses along ef must necessarily become equal to zero at a considerable depth below the surface, as indicated by the shaded area in the diagram. The average height, $H = n\ R$, of the shaded area depends on the relative density of the sand.

In the writer's judgment it may range between zero for loose sand and about $2R$ for dense sand. On this assumption the writer obtained the lines N_2 and N_3 for the relation between the depth and the ultimate bearing capacity of the pier. This result suggests that the ultimate bearing capacity increases approximately with depth and not with the square of the depth. As a matter of fact, the writer is unaware of any acceptable theory, or of any record of field observations that would sustain statement (*d*).

According to statement (*j*) the settlement due to a given load decreases rapidly as the depth of the loaded area beneath the adjacent ground surface increases. In order to apply a load on an area at a considerable depth below the surface it is necessary to dig a shaft. While the shaft is being excavated a stress relaxation develops within a zone whose width is several times the diameter of the shaft.[32] In the vicinity of the bottom of the shaft which later constitutes the loaded area and to a considerable horizontal distance from the bottom, the state of stress in the sand is practically independent of the depth of the shaft, provided the depth of the shaft exceeds several times the diameter of the shaft. The settlement of a loaded area depends to a large extent on the initial state of stress in the sand within the zone of potential plastic equilibrium. Since the state of stress in the sand below the bottom of the shaft is practically independent of depth, one would expect that the settlement of a pier at a given soil pressure at the base of the pier should also be practically independent of depth. This tentative theoretical conclusion is illustrated by Fig. 29. According to this figure the ultimate bearing capacity of the pier

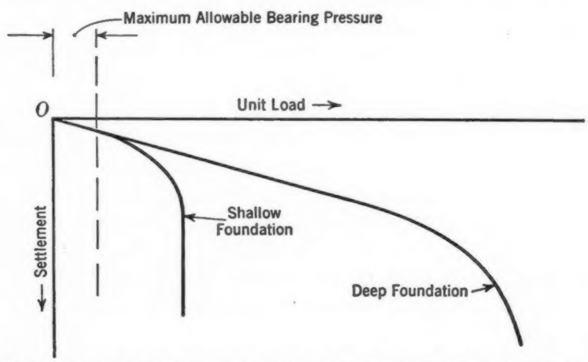

FIG. 29.—WRITER'S CONCEPTION OF THE INFLUENCE OF DEPTH OF THE FOUNDATION ON THE RELATION BETWEEN UNIT LOAD AND SETTLEMENT OF CYLINDRICAL PIERS

increases with depth, but the settlement for a given unit load is practically independent of the depth to the loaded area provided the load does not exceed the customary bearing value for the base of piers. The writer's experience in this matter is limited to a large-scale loading test which he made in 1928 on the bottom of a shaft on Houston Street, in New York, N. Y., at a depth of 55 ft below street surface. The bearing block covered the entire bottom of the shaft, from wall to wall. The results of this, and of all the other tests and observations

[32] "Theoretical Soil Mechanics," by Karl Terzaghi, John Wiley & Sons, Inc., New York, N. Y., 1943.

which he made in the shaft, corroborated the opinion illustrated by Fig. 29. Hence they are incompatible with statement (*j*). However, a single set of observations does not justify a final conclusion. Therefore, the writer would appreciate information concerning the field data on which Professor Casagrande's statement (*j*) is based.

Under the heading, "Soil Properties Responsible for Settlements: Stress-Deformation Characteristics of Clays and Other Cohesive Soils," the method for determining the preconsolidation load on clays receives considerable attention. This method was devised by Professor Casagrande in 1936.[33] The text of the original paper conveys the impression that the method is based on the following experimental investigation: Undisturbed samples of different clays have been consolidated in the laboratory under an arbitrary load. Then the load was removed and applied again. The curve which represents the renewed application of a load in the pressure, void-ratio diagram is known as recompression curve. After many recompression curves were obtained a graphical procedure was devised which made it possible to determine the laboratory preconsolidation load, if the corresponding laboratory recompression curve is known. To this point, the procedure was strictly empirical.

The next step consisted in using the procedure without any modification for the determination of a preconsolidation load that was applied by nature at an extremely low rate, thousands of years ago. Although this step constitutes a daring extrapolation, its implications have nowhere been discussed. In order to decide whether it is justified, it is necessary to compare the real value of the preconsolidation pressure produced by nature with those determined by means of the graphical method.

The real value of the preconsolidation load for overconsolidated clays can only be evaluated on the basis of geological evidence but, as a rule, geological evidence regarding the thickness of vanished strata leaves a wide margin for interpretation.

On account of the uncertainty of geological estimates the degree of accuracy of the graphical method can be evaluated only on the basis of records pertaining to clays for which the preconsolidation load is identical with the present overburden pressure. This condition is satisfied only for normally consolidated clays. Since these clays have never carried any load other than their own weight, the preconsolidation pressure increases approximately in simple proportion to depth.

In 1936 Professor Casagrande investigated a normally consolidated deposit of glacial clay about 100 ft thick and found that the graphically determined preconsolidation load below a depth of 50 ft is practically independent of the depth, whereas the true load increased in simple proportion to depth from about $1\frac{1}{2}$ tons per sq ft to about 3 tons per sq ft.[33]

Since he assumed that his graphical method is fairly reliable he suggested that the fissured rock beneath the clay may contain artesian water under an exceedingly high hydrostatic pressure. In 1941, D. M. Burmister, Assoc. M. Am. Soc. C. E., published the results of soil investigations of a soft clay deposit

[33] "The Determination of the Pre-Consolidation Load and Its Practical Significance," by A. Casagrande, *Proceedings*, International Conference on Soil Mechanics, Vol. III, 1936, pp. 60–64.

at the site of Flushing Meadow Park.[34] Between the depths of about 20 ft and 65 ft below the surface the actual overburden pressure on the clay increased from about 0.6 to 1.4 tons per sq ft. Within the same range of depth the overburden pressure determined by means of the graphical method decreased from 0.6 ton per sq ft at a depth of 20 ft to about 0.26 ton per sq ft at a depth of 65 ft; yet there was no evidence of the presence of artesian water in the sand beneath the clay.

To the writer's knowledge, no other set of data pertaining to drill-hole samples from normally consolidated clays has ever been published. Since the values obtained by means of the graphical method failed to agree with the known values of the preconsolidation pressure for the clays covered by the two published records, it seems unlikely that the agreement should be better in those instances in which the actual preconsolidation pressure is unknown.

On various occasions it has been stated that the graphical method has been applied successfully to the interpretation of the results obtained by tests on shaft and tunnel samples of normally consolidated clays. To eliminate the objection that the favorable results obtained on individual samples may be accidental, it would be necessary to make such tests on a set of samples which have been taken within the same stratum at very different depths; yet, thus far, no data of this type have been published.

The practical implications of the uncertainties concerning the evaluation of the preconsolidation load have been discussed elsewhere by the writer.[35] A systematic analysis of the striking discrepancies between reality and test results may increase, materially, the knowledge of physical properties of natural clays, but this promising source of information has not yet been tapped.

In discussing Fig. 10 the authors state:

> "From a large number of tests on different types of soils, it has been found that the preconsolidation pressure for most clays can be determined with a satisfactory degree of accuracy by means of the empirical method shown in Fig. 10 * * *."

Considering the records quoted, the term "satisfactory degree of accuracy" requires qualification. The writer has used the graphical method repeatedly to determine whether or not a clay has been heavily overconsolidated; but he has ceased to give any weight to the numerical results because both the published records and his own experience indicate that the difference between the computed and the real value for samples from the same drill hole may range between zero and several hundred per cent of the computed value. If the maximum preconsolidation load p_o exceeded the present overburden pressure p_1 by less than a few tons per square foot, the graphical method does not indicate whether p_o exceeded p_1 at all. In such instances, the writer relies entirely on geological evidence. During the seven years since the graphical method was devised, a wealth of data has been accumulated. Therefore the time seems to be ripe for a revision of the method on a broader empirical basis.

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[34] "Laboratory Investigations of Soils at Flushing Meadow Park," by Donald M. Burmister, *Transactions*, Am. Soc. C. E., Vol. 107 (1942), p. 187.

[35] Discussion by Karl Terzaghi of the paper, "Relation of Undisturbed Sampling to Laboratory Testing," by P. C. Rutledge, *Proceedings*, Am. Soc. C. E., June, 1943, p. 985.

Whatever the merits of the present graphical method may be, the research which centered about the preconsolidation load served a useful purpose. It gave an additional impetus to the development of the present methods of sampling and it broadened present knowledge of the physical properties of clays. Many of the refined procedures which grew out of these efforts will become obsolete because it will be found that the required expenditure of time and money is out of proportion to the practical value of what can be gained. Nevertheless the efforts were not wasted because they constitute part of the general exploration of the field and each one of them constitutes one more step toward the ultimate goal. The goal is the development of simple rules and practicable procedures which will reduce the hazards associated with the purely empirical methods of dealing with earthwork and foundation problems.

Under the heading, "Examples of the Application of Soil Mechanics to the Design of Building Foundations," the foundations of two new buildings in Boston are described. In the following discussion the new buildings will be briefly referred to as I (Liberty Mutual Insurance Company Building) and II (New England Mutual Life Insurance Building). Building I is supported by cylindrical piers whose enlarged base rests on a stiff clay crust with an average thickness of 10 ft. Fig. 30 shows a vertical section through the base of four

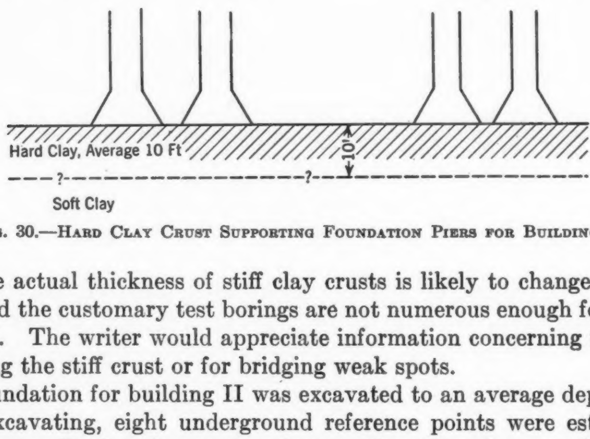

FIG. 30.—HARD CLAY CRUST SUPPORTING FOUNDATION PIERS FOR BUILDING

piers. The actual thickness of stiff clay crusts is likely to change from place to place and the customary test borings are not numerous enough for detecting weak spots. The writer would appreciate information concerning the method of surveying the stiff crust or for bridging weak spots.

The foundation for building II was excavated to an average depth of 35 ft. Prior to excavating, eight underground reference points were established in the stiff crust. During the process of excavation the points rose by distances up to 2.9 in. The observed settlement, to date, is roughly equal to the preceding rise of the stiff crust. In 1927 the writer designed a similar foundation. The weight of the building was roughly equal to the weight of the soil and water removed. The excavation penetrated through fine sand to a depth of 30 ft below the surface.[35] At a depth of about 14 ft below the bottom of the excavation the sand rested on the surface of a very thick stratum of soft glacial clay whose water content was close to the liquid limit. Before excavation was begun an underground reference point was established in the

[35] "Wellpoint Method for Handling Excavation of Foundation Pit at New Sewage Pumping Station, Lynn, Mass.," by Karl Terzaghi, *Journal*, Boston Soc. of Civ. Engrs., Vol. 14, 1927, pp. 389-397.

sand, a short distance above the soft clay. To the surprise of every one, the rise of the point was too small to be measured with an engineer's level. As a consequence, the settlement of the finished structure was also too small to be measured. However, the writer still believes that the heave of the bottom of the pit was only delayed. If the pit had been open for a couple of months, the heave might have started. Such a possibility is suggested by the well-known delay in the beginning of the swelling of homogeneous clays in tunnels and by the delay of the beginning of the settlement due to the consolidation of clay strata which has occasionally been observed.[36] There are no reliable means for predicting the amount of heave on the basis of test results. If no observations are made, even an important heave may pass unnoticed until it is too late to modify the design or the construction procedure in accordance with the reaction of the subsoil on the change of loading produced by excavation. Therefore the excavation for a deep, floating foundation should not be started without previously establishing a set of underground reference points.

Fig. 18 shows several vertical sections through the basements in building II. It would clarify the concept of the unsymmetrical continuous footings (Figs. 18 and 19(*b*)) if the authors would furnish some data concerning the assumptions on which the computation of the earth pressure on the outside of the basement walls was based.

The description of the two foundations constitutes a valuable contribution to current knowledge of the behavior of foundations on soft soil and it demonstrates again the intrinsic merits of the method of floating foundations.

[36] "The Actual Factor of Safety in Foundations," by Karl Terzaghi, *Structural Engineer*, Vol. 13, 1935.

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DISCUSSIONS

ILLINOIS WATER LITIGATION, 1940-1941

Discussion

BY MESSRS. L. R. HOWSON, A. M. BUSWELL, AND LLOYD M. JOHNSON

L. R. HOWSON,[4] M. AM. SOC. C. E.[4a]—In all of the litigation regarding diversion at Chicago, extending over a period of fifteen years, the experts for the Opposing States have never been critical of the engineering or construction of the Chicago Sanitary District's works. They have testified repeatedly of the high character of both the design and execution of that work. It is also only fair to state that some of the procedures eventually incorporated into the Sanitary District's program were first urged by the experts for the Opposing States. Such steps include:

(*a*) The recommendation that the Southwest, West Side and Calumet plants be of activated sludge rather than trickling filter type;
(*b*) That the Southwest and West Side projects be consolidated on the West Side site; and
(*c*) That provisions be made for lagooning sludge at the Southwest Side plant in case of breakdown in the sludge drying plant.

To secure a proper perspective of the contentions of the parties in the recent litigation covered by Mr. Pearse's paper, reference is made to the opinion of the U. S. Supreme Court delivered by Chief Justice Taft on January 14, 1929:

> "The Sanitary District authorities relying on the argument with reference to the health of its people, have much too long delayed the needed substitution of suitable sewage plants as a means of avoiding the diversion in the future. Therefore, they cannot now complain if an immediately heavy burden is placed upon the District because of their attitude and course. The situation requires the District to devise proper methods for providing sufficient money and to construct and put in operation, with all reasonable expedition, adequate plants for the disposition of the sewage through other means than lake diversion."

The Supreme Court then instructed the Special Master, the Hon. Charles Evans Hughes, "to determine the practical measures needed to effect the

NOTE.—This paper by Langdon Pearse, M. Am. Soc. C. E., was published in December, 1942, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: May, 1943, by Willem Rudolfs, M. Am. Soc. C. E.

[4] Cons. Engr. (Alvord, Burdick & Howson), Chicago, Ill.

[4a] Received by the Secretary May 3, 1943.

object just stated and the period required for their completion." After an exhaustive series of hearings in which the experts for the Sanitary District estimated that the sewage disposal program would require the expenditure of $176,000,000 and fifteen years in which to build it, and the engineers for the lake states estimated $76,000,000 to $82,250,000 and four to eight years for construction, the Master adopted a construction period of nine years from the date of the order. The project is now (1943) substantially completed. Approximately $80,000,000 have been expended for the sewage disposal program set up in the Master's report. Due to the economic depression delaying the start of construction, most of the program was condensed into a six-year construction period.

Attention is called to Table 3 which discloses that, during 1939, the Sanitary District discharged 53,970 tons of sludge and grit (on a dry basis) into the drainage canal, and that the total volatile solids discharged into the canal in the same year (Table 4) was 115,658 tons. Expressed in another way, from two to three million tons of liquid sludge which had been removed in the treatment plants were dumped back into the channel during 1939 in addition to the solids necessarily carried by the plant effluents. The surprising thing is not that the channel did smell but that it did not smell worse.

Some of the hearings were held in the city hall in Joliet. This building is near the drainage channel. The hearings were conducted during the hottest week that has ever been recorded in the Joliet weather records so that the setting was perfect for severe odors. During the period when witnesses were testifying as to the intensity of the odors, the wind was frequently blowing into the court room directly from across the channel. There is such a thing as "judicial notice."

The Special Master records his impressions of the odor situation as he observed it on a boat trip over the Brandon Pool one evening when the hotel thermometer after his return at 9:00 p.m. registered 92°. The following excerpt is from the Master's report:

> "After returning from this inspection trip I noted at 11:30 P.M. in my room at the hotel (which was about 4 blocks from the Waterway) an odor which lasted a minute or two; a few minutes later I got another odor for a minute or two, and then detected no more odors. I slept four nights at Joliet and was not troubled by odors. The only trouble I had in sleeping was due to the heat, which was so great that I kept an electric fan constantly going all night. The City Hall, in which the hearings were held, was about three blocks from the Canal on the east side; I detected an odor only on one day and then only for a few minutes. The four days I spent in Joliet were the hottest of the summer, the temperature reaching 103 degrees on July 25th."

In referring the matter to Monte M. Lemann as Special Master, the Supreme Court in its per curiam opinion (309 U. S. 569) prefaced the quotation referred to by Mr. Pearse with this statement:

> "The State of Illinois has failed to show that it has provided all possible means at its command for the completion of the sewage treatment system as required by the decree as specifically enlarged in 1933 (289 U. S.

395, 710). No adequate excuse has been presented for the delay. Nor has the state submitted appropriate proof that the conditions complained of constitute a menace to the health of the inhabitants of the complaining communities or that the state is not able to provide suitable measures to remedy or ameliorate the alleged conditions without an increase in the diversion of the rights of the complainant states as adjudged by this court."

Accordingly, the position of the Opposing States in this litigation was that their function was simply to suggest various "remedial or ameliorating measures" and that in view of the aforementioned earlier Supreme Court opinions, the cost of such measures was not a consideration. The Opposing States were not concerned as to what steps Chicago or the State of Illinois might take so long as they did not involve increased diversion from Lake Michigan. All engineers were agreed that the Sanitary District should complete its construction program at the earliest possible date. If the State of Illinois considered the Joliet nuisance sufficiently acute or inimical to health, the Opposing States contended that it was obligatory upon Illinois to adopt any or all "remedial or ameliorating measures" at its expense and without detriment to the Opposing States. It should be recalled that the Opposing States did not initiate the 1941 hearings. The State of Illinois was plaintiff.

It is noticed, in Table 7, that Illinois takes credit for No. 4; namely, keeping all sludge out of the Main Channel. At the time of the hearing approximately half of the sludge that had been separated out in the treatment processes was being discharged back into the drainage channel. The experts for the Opposing States, in their suggestion of remedial measures, recommended the immediate cessation of that discharge and that the sludge be deposited in lagoons until the dryers were adequate. Initially the Illinois experts testified that lagooning of sludge was impracticable and could not be adapted to Chicago conditions. Subsequently, however, that position was changed and lagooning, in fact, was provided by the Sanitary District before the Master's report was completed.

Table 7 does not include No. 2, "Activated sludge treatment for West Side Imhoff tank effluent," as a remedial measure in the "Opposing States" column. However, this was one of the measures urged by the Opposing States experts for immediate undertaking and opposed by Illinois as requiring more time than estimated by the Opposing States. In commenting on these opposing views the Master quoted from Special Master Hughes,

> "Much time can be saved or lost in large building operations according to the attitude which is taken as to the importance of early completion. In the present case the Court has already laid down the requirement that the work shall proceed 'with all reasonable expedition.' "

Remedial Measure No. 9 "budgeting of the diversion" was also the initial suggestion of the experts for the lake states. This, too, was a matter on which all parties came to agreement during the progress of the case.

As to most of the other "remedial or ameliorating measures" suggested as available the Master considered them as not "feasible," worth while, or necessary. His report stated:

"The people in Joliet and Lockport have submitted to the conditions which prevailed in the summers of 1939 and 1940 without serious consequences to health. If relief is denied for the years 1941 and 1942, the authorities of the Sanitary District may be spurred to increased efforts in the treatment of Chicago sewage. The hearings before me have already resulted in the putting into effect of provisions as to lagooning of incompletely treated sewage which had not previously been adopted."

From the fact that there have been four Supreme Court hearings on the diversion of water from Lake Michigan for sewage disposal purposes, it is evident that there have been some divergent views. However, in all of these cases the argument of Illinois has been for diversion. Previous cases have seen much testimony relative to diversion for power, diversion for navigation in the Illinois Waterway, and even the Mississippi River, but in this case the diversion was asked as a direct health measure. The Master recommended against, and the Supreme Court denied, the petition for increased diversion. The Master stated "Every increased diversion from the lakes removes a stimulus to Illinois to speed the work." The Master reported a nuisance but no menace to health.

A. M. BUSWELL,[5] ESQ.[5a]—It is fortunate that Mr. Pearse has prepared for record a summary of the rather unique litigation which, as he states, involved the question, "When is pollution a menace to health" rather than a question of nuisance abatement.

Although much of the testimony related to evidences of nuisance and the relative effectiveness of various means of relieving odors, the plea was that the situation was so extreme as to have an actual effect on health. On this, the main point of the controversy, the Court stated that (see "Conclusions as to Health Conditions"):

"In the present case it is Illinois itself which is creating the nuisance of which it complains and of which it seeks to be relieved by water which has in effect been adjudged by the Court to belong to the opposing States.* * *

"My conclusion is that the facts proven do not establish any menace to the health of the inhabitants of Joliet and Lockport or elsewhere along the Waterway requiring an increase in diversion in water from Lake Michigan."

Since one of the principal reasons for increased diversion was to flush out sludge deposits which it was contended had accumulated as a result of decreased diversion, it is of importance to note (Table 3) that the discharge of sludge to the channel increased from some 30,000 tons in 1934 to some 50,000 tons in 1939. As a result of the hearings, this was reduced to 20,000 tons in 1940 and to 0 in 1941. This elimination of the discharge of sludge to the channel was urged by the opponents of diversion as the most effective immediate remedial measure, and it is gratifying to note that the suggestion was acted upon so promptly.

[5] Chf., State Water Survey Div., Urbana, Ill.

[5a] Received by the Secretary May 7, 1943.

The presentation of these and other suggested ameliorating procedures by Mr. Pearse may be misleading to some readers. The Opposing States did not contend that any one item in their program (Table 7) would completely relieve the situation. They did submit that the effect of combining the permanent cessation of sludge discharge with such temporary measures as chlorination and chemical precipitation at certain plants to improve efficiency, and chlorination where dissolved oxygen deficiencies were imminent, would give greater relief than increased diversion. This contention was supported amply by the results of the 10-day flushing test (see discussion of Table 8) that swept some 500,000 cu yd of sludge into Brandon Pool, increasing the pollution at that critical point.

The page and a half of testimony on chlorination may confuse persons not familiar with the subject. The hesitancy of those offering testimony to state definite requirements and predict quantitative results was due to their realization of the complicated nature of the chlorination reaction. Complete or "break point" chlorination requires relatively large amounts of chlorine but hydrogen sulfide and other odorous compounds are chlorinated far below the break point. It is important to note that all agreed that chlorination was an effective means for odor control. Under the heading, "Available Remedial Measures," the writer is quoted as admitting that "dilution reduces the B.O.D. more certainly than chlorine." Actually the writer's testimony was that the reduction of B.O.D. by dilution can be calculated more accurately in the light of present knowledge than the reduction by chlorine can be calculated. The writer is further quoted (heading, "Supply of Additional Oxygen through the Production of Nitrate by Increased Use of Air at North Side and Calumet Works") as admitting that "there would be odors even if nitrates were present." He wishes here to state that the presence of nitrates is incompatible with putrefactive odors. If the nitrates are not present in sufficient quantity they would be reduced, after which odors would appear.

The summary of testimony on cascades fails to point out that the increase in dissolved oxygen after aeration is not a true measure of the results accomplished. Septic compounds are largely volatilized or oxidized by aeration, thus reducing the B.O.D. by several times the amount of actual oxygen increase. There is no question that aeration is effective, although it may not be worth "$1,500 per day to the Sanitary District."

Emphasis should be placed on the fact that there was no criticism of the final program of the Sanitary District of Chicago for relieving the situation permanently. Both parties to the litigation urged a speeding up of this program as the best solution of the problem. It is apparent that the Special Master came to the same decision. This is brought out forcefully in the final paragraph of his conclusions quoted in the last paragraph of the Appendix to Mr. Pearse's paper.

LLOYD M. JOHNSON,[6] ESQ.[6a]—Mr. Pearse's paper records clearly the proceedings in this case. It seems unfortunate that it was necessary for Illinois

[6] Commissioner of Streets and Electricity, City of Chicago, Chicago, Ill. (formerly, Engr. of Maintenance and Operation, The Sanitary District of Chicago).

[6a] Received by the Secretary May 27, 1943.

to prove a menace to health in the conditions existing in the Waterway, in order to obtain a temporary increased flow. To the layman the existence of nuisance would warrant such increase to relieve the conditions.

In view of the accumulations of sludge in the Waterway, and the persistence in oxygen demand of these deposits over several years, a temporarily increased diversion greater than 1,500 cu ft per sec appears extremely desirable during the summer months until the Sanitary District sewage treatment construction program can be completed. This increase would be most effective if taken during the summer months on a schedule similar to that outlined in the modified petition.

A plant of the unprecedented size and complications of the Southwest works of the Sanitary District requires a lengthy "tuning up" period. Scarcity of materials and the priority situation have worked against keeping the plant at maximum operating capacity. Due to limitations in sludge handling capacity the plant has operated so that the reduction of B.O.D. has been approximately 60%. The war has stopped work leading to construction of additional facilities for the complete treatment of all sewage from the Southwest area, and the activated sludge treatment of the West Side Imhoff tank effluent.

Under conditions in 1940–1941 the over-all B.O.D. reduction was approximately 60% for the sewage flow of the four major sewage treatment works of the Sanitary District. In 1938 (the last year of the diversion of 5,000 cu ft per sec and before the Southwest works was placed in operation) the over-all reduction was roughly 35%. With activated sludge treatment for 400 mgd at the Southwest works the reduction would average about 80%, and with such treatment for the West Side flow also, which is now only settled in Imhoff tanks, the value would be 90%. These figures do not take account of the effect of storm flows.

Table 10 shows a comparison of the residual sewage untreated, on a population basis, and the dilution afforded before and after the reduction in diversion,

TABLE 10.—COMPARISON OF RESIDUAL SEWAGE UNTREATED, ON A POPULATION BASIS

Year or condition	Percentage treatment	Residual population untreated	Diversion, in cu ft per sec: Total population	Diversion, in cu ft per sec: 1,000 population
1938	35	4,220,000	5,000	1.18
1941	60	2,605,000	1,500	0.58
Treatment[a]	80	1,302,000	1,500	1.15
Treatment[b]	90	651,000	1,500	2.30

[a] Activated sludge treatment for 400 mgd at Southwest works and Imhoff tank treatment at West Side works.

[b] Activated sludge treatment for the flow of the entire Sanitary District.

based on a total population equivalent (human plus industrial equivalent) of 6,512,000, which actually occurred in 1941. The value was probably less in preceding years, but 1941 was the first full year for which data were available.

Thus, it is seen that in 1940–1941 the dilution available was about one half that in 1938, before the reduction in diversion. The fact that the lower diversion contained a much higher proportion of direct runoff is disregarded.

Regarding the suggested remedial measures it is fortunate that funds were not used to install chlorinating apparatus or chemical treatment at the West Side works. Any other funds expended for temporary measures would have been largely wasted. In the light of developments since the hearings it is fortunate that the use of chlorine was not adopted, as the war conditions would probably have made it impossible to obtain chlorine for such purpose.

In closing the comments on Mr. Pearse's paper, it is now clear that the most prompt and effective method to ameliorate the condition would have been increased diversion. Unless an emergency order is issued granting increased diversion, the condition of the Illinois Waterway must remain as it is for the duration of the war and for some time thereafter.

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DISCUSSIONS

PENDLETON LEVEE FAILURE

Discussion

BY MESSRS. K. TERZAGHI, T. A. MIDDLEBROOKS, AND D. P. KRYNINE

K. TERZAGHI,[4] M. AM. SOC. C. E.[4a]—The Pendleton Levee failed along a surface of sliding that was mostly within a fairly horizontal stratum of clay. During the fifteen years since 1928 several major failures of a similar nature have occurred. The failure of the Lafayette Dam in California in September, 1928,[5] was followed by the failure of the embankment for the water supply reservoir, Chingford II, north of London, in July, 1937,[6] the failure of the Marshall Creek Dam in Kansas in September of the same year,[7] and the failure of a flood-protection dike within the city limits of Hartford, Conn., in August, 1941.[8] The loss of capital due to these failures amounted to several million dollars and the frequency of the failures of fills above horizontal clay strata does not yet show any tendency to decrease. Therefore the subject of the paper by Messrs. Fields and Wells deserves the attention of every engineer who deals with the construction of earth dams or embankments.

The paper is divided into three parts: (1) The results of the soil tests and field observations; (2) an analysis of the failure; and (3) the description of a graphical method for computing the stability of fills on horizontal clay strata.

The graphical stability computation consists essentially in replacing the true surface of sliding by one plane and one or two cylindrical surfaces. Such ideal surfaces are known as composite surfaces of sliding. The method seems to have originated before 1933 in the Preussische Versuchsaustalt für Wasserbau aud Schiffbau in Berlin, Germany, and it has been used repeatedly with minor modifications in both the analysis of failures and in the design of new dams. Thus, for instance, the failure of the Chingford embankment was

NOTE.—This paper by Kenneth E. Fields and William L. Wells, Assoc. Members, Am. Soc. C. E., was published in December, 1942, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: December, 1942, by Ralph B. Peck, Jun. Am. Soc. C. E.

[4] Winchester, Mass.

[4a] Received by the Secretary April 26, 1943.

[5] "Reconstruction of Lafayette Dam Advised," *Engineering News-Record*, Vol. 102, 1929, pp. 190–192.

[6] "The Analysis of the Failure of an Earth Dam During Construction," by L. F. Cooling and H. Q. Golder, *Journal*, Institution of Civ. Engrs., No. 1, November, 1942, pp. 38–55.

[7] "Foundation of Earth Dam Fails," *Engineering News-Record*, Vol. 119, 1937, p. 532.

[8] "Foundation Failure Causes Slump in Big Dike at Hartford, Conn.," *Engineering News-Record*, Vol. 127, 1941, p. 142.

analyzed by the writer in 1938 by means of this method. The method is accurate enough for any practical purpose, provided fairly accurate values have been chosen for the soil constants. Therefore, only the method for determining the soil constants is properly a topic for discussion.

The failure analysis in the paper under discussion is based on the tacit assumption that the clay stratum does not contain any layers of cohesionless material. The c-value of the material within the stratum is equal everywhere to 0.04 ton per sq ft, and the ϕ-value is equal to 19°. The value of ϕ has been determined by means of quick shear tests. On the basis of these data, the authors found that the factor of safety of the land side of the embankment was roughly equal to unity at the instant of failure. However, according to the results of a similar computation based on the same values, the factor of safety of the river side where no failure occurred was even less than unity. This fact suggests that the correct result which was obtained for the land side was merely due to chance. Further, it should be noted that the computation of the shearing resistance was made by means of a formula (Eq. 1) which contains the pore water pressure u. The value ϕ which appears in such an equation should be determined by means of slow, and not of quick, shear tests. The slow tests yield an angle of shearing resistance ϕ_1, a much higher value than the quick tests. The computation should have been made on the basis of the higher value, ϕ_1. Under this condition it would have led most likely to the conclusions that the factor of safety of the land side, on the day of the failure, was equal to about 1.4 and that of the river side was equal to about 1.2 (writer's guess). In other words, the computation would not account for the failure of the land side of the levee.

This conclusion leads to what appears to be a weak point of the present attitude of engineers toward stability computations in general. Since about 1928 the attention of research men has been concentrated almost exclusively on the mathematics of stability computations and on refining the technique of sampling and testing. The results of these efforts were very useful and illuminating. At the same time they created a rather indiscriminate confidence in laboratory test results. In some instances confidence is warranted, but, in many others, it can lead to erroneous conclusions, because the attention of the investigator is diverted from what is really essential. The failure of the Pendleton Levee demonstrates that the essentials in the stability conditions are by no means always within the reach of the laboratory eye and of the slide rule.

When examining Fig. 5 the reader will notice that a strip of the fill, about 50 ft wide, on the land side of the center line subsided. The remainder of the fill, beneath the lower part of the land-side slope, moved bodily forward, and the slope angle remained almost unchanged. It should be noted further that the settlement plate beneath the area of subsidence descended to a depth of more than 5 ft below the level of the adjoining settlement plates.

These observations suggest that the failure occurred as indicated in Fig. 17. The fill beneath line ab settled into a trough-like depression. The block of fill, sand, and clay beneath the slope section bc moved bodily on a plane ef through a distance of about 6 ft to the right. The horizontal pressure exerted by the moving mass bceg caused a horizontal shortening and an upward bulge

of the block cdgf whose presence interfered with the movement. The absence of any appreciable deformation within the mass bceg indicates that the resistance against sliding on the base ef of this mass was practically equal to zero. If the weight of the overburden above a horizontal section through a mass of homogeneous clay is carried by the pore-water pressure, the shearing resistance along this section is equal to the cohesion of the clay. Therefore, the type of failure illustrated by Fig. 17 suggests that the surface of sliding ef

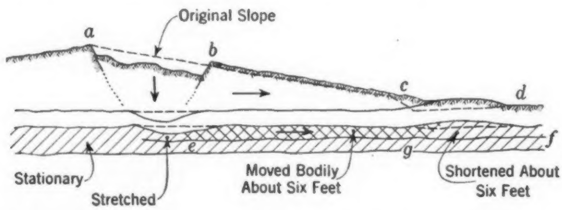

FIG. 17.—MASS MOVEMENTS AND DEFORMATIONS ASSOCIATED WITH FAILURE OF PENDLETON LEVEE

was not within the clay but within a fairly continuous layer of sand or silt which separated the lower stationary part of the clay stratum from the upper, moving one. Since the piezometer readings showed that the pore-water pressure was equal to, or greater than, the overburden pressure over the entire area ef, the frictional resistance in the layer was equal to zero. The presence of fairly continuous layers of highly permeable material in the central part of the clay stratum is demonstrated by Fig. 10. In this figure the dashed lines marked February 5, 1940, and February 14, 1940, indicate a hydraulic gradient which was directed from the boundaries of the clay stratum toward the central part of the stratum. Hence, at the points where the piezometers were located, the excess water drained toward the central part of the stratum. No such flow possibly could have occurred unless the central part of the stratum contained some layers through which the water could escape in horizontal directions.

The only point that requires any further discussion is the origin of the excess hydrostatic pressure in the clay above and below section ef, Fig. 17. Under the heading, "Analyses of Failure," the authors state that "* * * some of the excess pore pressures were no doubt caused by the remolding of the clay in areas where shearing stress exceeded strength * * *." In this connection it should be noted that the deformation of the clay on both sides of section ef of the surface of sliding remained insignificant until the failure occurred. Hence, if any of the excess pore-water pressure were really due to remolding, these pressures must be considered not a cause but a consequence of the failure. Yet, no such hypothesis is required, because the excess pore-water pressures in the clay beyond the toes of the fill can be explained adequately by the sedimentary origin of the clay. The permeability of clays that were placed in horizontal layers is much greater in horizontal directions than in vertical ones. Therefore, the excess pore-water pressure produced by a local surcharge spreads from the area covered by the surcharge in both directions. The mechanics of this process are illustrated by Fig. 18, which shows a horizontal clay stratum between two sand strata. The surface of the upper sand stratum carries a

fill with inclined sides. If the clay stratum is perfectly homogeneous, the excess water drains out of the clay in an almost vertical direction and the distribution of the excess pore-water pressure over a horizontal section through the clay is as shown in Fig. 18(*a*). The excess pore-water pressure, represented by the vertical distance between the water table and the dashed line, decreases from a maximum beneath the crest of the fill to almost zero beneath the toes. On the other hand, if the clay stratum contains fairly continuous layers of coarse silt, part of the excess water drains out of the clay into the silt, leaving the silt beyond the area which is occupied by the fill. The clay stratum be-

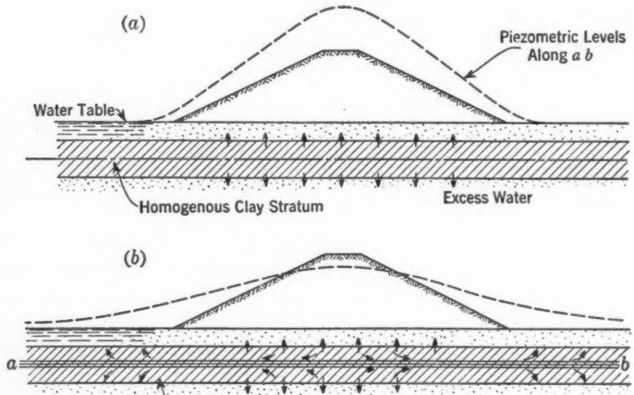

FIG. 18.—INFLUENCE OF SILT PARTINGS IN A CLAY STRATUM ON THE DISTRIBUTION OF EXCESS HYDROSTATIC PRESSURE AT THE MIDHEIGHT OF THE STRATUM

neath the Pendleton Levee contained such layers, because, according to Fig. 10, part of the excess water drained out of the clay toward a plane not far from midheight of the stratum. The distribution of the excess pore-water pressure over a horizontal section through the layer of silt is shown by the dashed line in Fig. 18(*b*). The presence of the layer reduces the excess pore-water pressure beneath the central part of the fill and increases it within the belts beneath the toes.

The preceding failure analysis leaves no doubt that the clay stratum contained fairly continuous silt or sand partings and that these partings played a decisive rôle in the mechanics of the failure. On account of the presence of these partings, the resistance against sliding was equal to zero over two broad areas in silt or sand partings in the clay, beneath the toes of the fill. The safety of the fill with respect to sliding depended primarily on the position of the inner boundaries of these areas with reference to the center line of the fill; yet the position of these boundaries depended on minor geological details that could not be ascertained by any practicable means of exploration or computation. Therefore, one cannot predict which side of the fill will fail first, unless the stability conditions on both sides of the center line are conspicuously different.

At the instant of failure the mass of soil above one of the two frictionless areas moved out bodily. The outward movement was resisted only by the passive earth pressure of the mass of soil above the frictionless area, beyond the toe of the fill. Within the clay stratum the passive resistance of the clay is equal, roughly, to the average unconfined compressive strength of the clay.

The inevitable uncertainties involved in estimating the distribution of the excess pore-water pressure on horizontal sections within sand or silt partings eliminate the usefulness of refined theoretical or experimental investigations in connection with the design of fills above horizontal clay strata of sedimentary origin. This is demonstrated by the disappointing results of the theoretical investigations contained in the paper. The most important requirement for adequate design is a thorough knowledge of the type and character of the stratification of the clay stratum, of the initial shearing resistance of the weakest layer, and of the average unconfined compressive strength of the entire layer at different points.

The safest way to secure information concerning the stratification is to inspect the sides of amply large test shafts closely. If the excavation of test shafts is impracticable, a careful study should be made of continuous cores of the stratum. To estimate the initial shearing resistance of the weakest layers of the clay and the average unconfined compressive strength, compressive-strength diagrams are needed which show the variation of the unconfined compressive strength in a vertical direction in different parts of the clay stratum. The samples can be obtained by means of 2-in. seamless tubes. One compressive-strength value should be secured for every 4-in. section of the cores. One compression test requires about 10 min. However, after an experienced experimenter has made some forty or fifty tests on very different samples from the same stratum, he is able to estimate the compressive strength of any sample from this stratum. Once he has acquired this capacity he needs to test only the softest and the stiffest samples. The strength of the intermediate ones can be estimated. The compressive strength profiles should be supplemented by water-content diagrams, because these diagrams facilitate the identification of individual layers encountered in different drill holes. Consolidation tests and Atterberg limit tests should be made on representative samples, primarily for the purpose of securing general information concerning the clay. The test results can be used for computing the rate of consolidation, only if the ratio between the average coefficient of permeability of the clay in a horizontal and in a vertical direction is known.

In conclusion the writer wishes to emphasize once more the outstanding practical value of the observations made on the Pendleton Levee. The published data give a clear picture of the processes that precede the failure of a fill on a horizontal clay stratum, and they illustrate impressively the uncertainties involved in any attempt to predict the failure conditions. In connection with the design of foundations, a clear realization of the inevitable gaps in current knowledge is the beginning of wisdom, and these uncertainties can be realized only on the basis of comprehensive field observations such as those made by the authors. It is hoped that Messrs. Fields and Wells will supple-

ment the information contained in their paper by further information concerning the structure and stratification of the clay stratum. It is also hoped that, some day, they will be able to supplement their record of the failure by several water-content profiles and compressive-strength profiles of the clay. The record is by far too valuable to remain incomplete.

In connection with the technique of the field observations, many readers, including the writer, would appreciate information concerning the technique of sealing the drill holes for the piezometric tubes. The paper does not mention the grain size of the commercial bentonite product used. If the product is very fine-grained, the seal may be imperfect, and, if it is very coarse-grained, the swelling pressure is likely to create a local disturbance of the hydrostatic pressure conditions in the clay. The data in the paper suggest that some of the measured pore-water pressures are too high, because, beyond the toes of the fill, the pore-water pressure cannot become greater than the total weight of the material above the seat of the pressure. As soon as the pore-water pressure in a sand or silt parting becomes equal to the weight of the overburden, the overburden is lifted gradually by the accumulating water; but the pore-water pressure remains constant.

T. A. Middlebrooks,[9] Assoc. M. Am. Soc. C. E.[9a]—The levee failure in the Vicksburg Engineer District is an interesting experiment which the writer has followed from its inception. The authors are to be complimented on their paper. Installation of the hydrostatic pressure points was part of a general program to obtain information on the development and distribution of hydrostatic pressure in unconsolidated silt and clay strata when subjected to a superimposed load.

Evidence, accumulated over a period of years, has convinced the writer that excess hydrostatic pressure is a major factor in the design of embankment over unconsolidated foundations. During the Mississippi levee construction program after the 1927 flood, the writer had the opportunity of observing numerous foundation failures over soft clay foundation. After developing an empirical method of designing levees over such a foundation and using it satisfactorily for levees constructed at a normal rate, the writer was surprised to find that it was not satisfactory for a levee constructed at top speed. On a levee which had to be completed before a flood, the width of the berm, or total base width of the levee, had to be more than doubled to provide a stable section for such rapid construction.

Hydrostatic pressure observations are considered essential as a check on design and control during construction. The rate of consolidation, which is one of the most important variables in design, can be checked closely during construction and, if excessively high pressures are developed, the rate of construction can be decreased or the slopes flattened to insure a stable structure. Such observations are inexpensive insurance against possible trouble. In most cases, there is no need for elaborate installation. These observations can be made by a simple installation of small, open-end pipes set in the clay at the desired elevation.

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[9] Prin. Engr., Corps of Engrs., War Dept., Soil Mechanics Unit, Washington, D. C.

[9a] Received by the Secretary May 19, 1943.

As construction control measures and for research purposes, the War Department has made numerous installations for observing hydrostatic pressure under earth dams and levees in the past few years. Two recent installations under large earth dams—one in a silt stratum and one in a micaceous clay stratum—confirmed the conclusions indicated by laboratory tests that these strata would consolidate rapidly during construction. In both of these cases, the building up of excess hydrostatic pressure was small and decreased rapidly when construction of the embankment was delayed due to weather or other reasons.

The writer cannot agree with the authors' use of the "consolidated-quick" test for determining the strength of the clay where the excess hydrostatic pressure readings are available. In this type of test, there is a building up of hydrostatic pressure in the sample due to the rapid rate at which the sample is sheared. In using these test results, the effect of excess hydrostatic pressure is doubly accounted for: First, in the quick shear strength value; and, second, in the actual hydrostatic pressure observed in the clay. The result is that a lower factor of safety is obtained than if the net (grain-grain) strength had been determined. The writer also disagrees with the use of the maximum strength value in place of the ultimate or minimum strength value. The result, in this case, is that a higher factor of safety is obtained. Under these conditions, it would appear that these two factors were more or less compensating, thus accounting for the close check obtained.

The stability analysis in such cases should be based on the shearing strength determined from a "consolidated-slow" test, where excess hydrostatic pressure is not allowed to develop in the sample, and the net strength is determined. This would appear to be elementary, since in the analysis the acting forces are divided into two distinct parts: First, the measured hydrostatic pressure; and, second, the net force, determined by subtracting the hydrostatic force from the total acting force. Then the strength to be used would be the net strength, determined from the "consolidated-slow" test. In addition, the minimum strength should be used, since in all failures of this type there is sufficient progressive movement before the full failure plane is developed. Thus the minimum strength is existing all along the so-called failure plane or zone. The authors recognized this fact but failed to employ it in their analysis.

In 1934 and 1935, the writer had an opportunity to investigate the deformation of clays in connection with the design of the Fort Peck Dam on a deep clay formation.[10] His conclusions were borne out by the satisfactory performance of the dam over this clay strata. Model experiments in the laboratory, combined with previous experience, convinced the writer that the "yield point" strength (about equal to the minimum strength) should be used instead of the maximum strength. Fig. 19 shows typical results of these model experiments. It will be noted that the clay did not develop a true failure plane even though the deformation was sufficient to term the clay mass "failed."

The circular slide method appears to give results that are satisfactory in this case, and it is generally considered accurate enough for all practical

[10] "Foundation Investigation of Fort Peck Dam Closure Section," by T. A. Middlebrooks, *Proceedings*, International Conference on Soil Mechanics and Foundation Eng., Harvard Univ., Cambridge, Mass., 1936.

purposes. However, a better understanding of the factors involved in problems of this nature can be obtained by the elastic-theory method as outlined by the writer in the previously mentioned paper[10] and in a subsequent one on the failure of the Fort Peck Dam.[11]

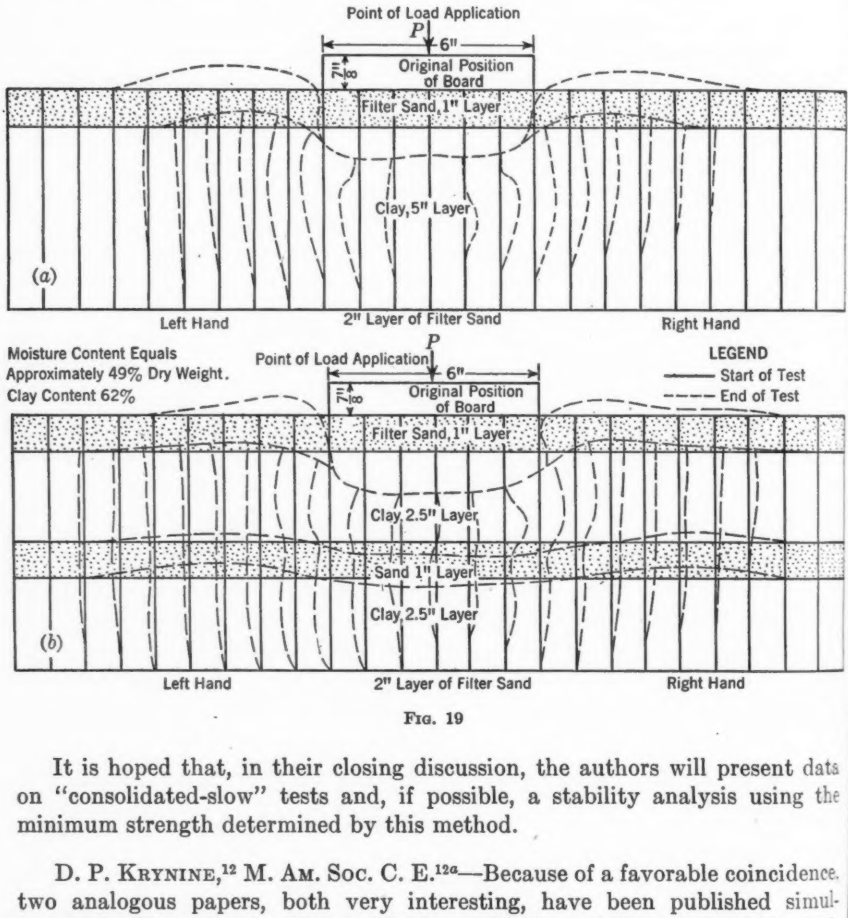

FIG. 19

It is hoped that, in their closing discussion, the authors will present data on "consolidated-slow" tests and, if possible, a stability analysis using the minimum strength determined by this method.

D. P. KRYNINE,[12] M. AM. SOC. C. E.[12a]—Because of a favorable coincidence, two analogous papers, both very interesting, have been published simultaneously: The paper by Messrs. Fields and Wells, and that on the Chingford, Essex (England), earth dam failure.[6] At the time of failure both structures were approximately of the same height (32 ft at Pendleton and 26 ft at Chingford). The soil conditions are also very much alike. Those at Pendleton are shown in Fig. 1 and those at Chingford are: Soft clay varying between 0 and 12 ft, usually about 3 ft thick; a layer of "river ballast"; and finally the so-

[11] "Fort Peck Slide," by T. A. Middlebrooks, *Transactions*, Am. Soc. C. E., Vol. 107 (1942), p. 72

[12] Research Associate in Soil Mechanics, Dept. of Civ. Eng., Yale Univ., New Haven, Conn.

[12a] Received by the Secretary May 24, 1943.

[6] "The Analysis of the Failure of an Earth Dam During Construction," by L. F. Cooling and H. Q. Golder, *Journal*, Institution of Civ. Engrs., No. 1, November, 1942, pp. 38–55.

called "London clay." The total thickness of both the "river ballast" layer and the upper soft clay layer is from 10 to 30 ft. There was a puddle core at Chingford and no core at Pendleton. In both cases the failure occurred in the same way. A large part of the failure line (shear surface) is practically horizontal and is within the clay layer underlying the structure (Figs. 11 and 20[13]).

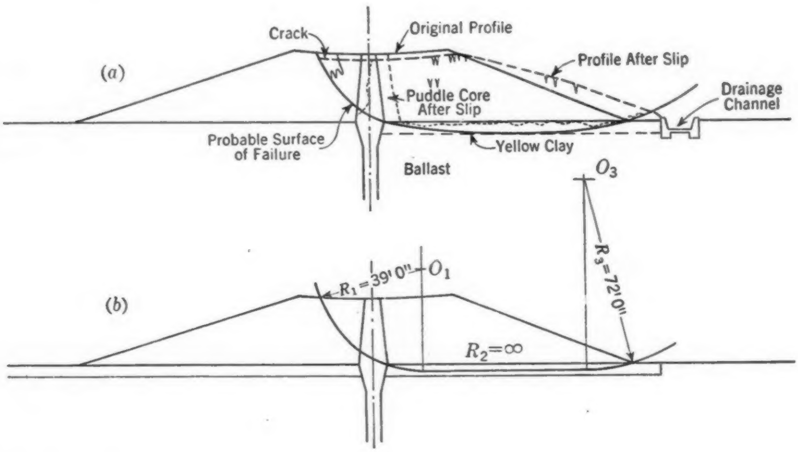

Fig. 20

The slope of the failure line close to the center line of the structure is rather steep, and there is a sinking of the crown in both cases. It may be concluded from the movement of the settlement plates at Pendleton (Figs. 3(*a*) and 5) that the clay compressed between two sand layers was squeezed out toward the toes. Apparently, in Chingford there was an analogous situation.

Horizontal Movement of Pore Moisture.—Fig. 21 represents a hypothetical clay layer between two sand layers. Points A and A′ are on a vertical line

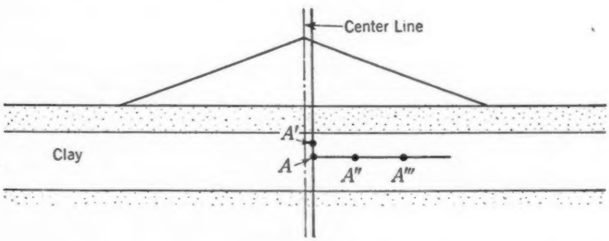

Fig. 21

very close to the center line of the dam, whereas points A, A″ and A‴ are on a horizontal line. During the process of consolidation, the pore-moisture pressure at point A is greater than at point A′; and the pressure at point A is

[13] *Ibid.*, Figs. 4 and 5*b*.

greater than at point A″ because of the difference in the heights of the dam. In its turn, the pressure at A″ is greater than at A‴. Hence at point A there is a hydraulic gradient in the vertical direction driving moisture from the clay into the sand; and, at point A, there is a hydraulic gradient in the horizontal direction driving moisture from the center of the dam toward the toe. In reality, moisture driven out of the clay layer follows curvilinear paths. For the sake of simplicity, however, vertical and horizontal components of this movement will be considered.

The behavior of that moisture which moves horizontally depends mostly on the capacity of the sand layers above and below the clay to take up moisture from the clay, especially close to the toes of the structure. If drainage at the toe is poor (saturated sand, or rainy weather, or both), moisture will accumulate close to the toes in an adiabatic way (that is, without leaving the mass).

Difference in Vertical and Horizontal Permeability.—If the coefficient of horizontal permeability in the clay layer (Fig. 21) is considerably larger than the coefficient of vertical permeability, moisture arriving horizontally at the toes will not be satisfactorily drained vertically in the clay layer itself. This can be proved easily by computing the discharge at the toes, in both horizontal and vertical directions, using the Darcy formula with two different coefficients of permeability. All measures tending to relieve pore pressure at the toes—particularly the drainage of the clay mass by vertical sand wells—contribute to the stability of the structure.

Character of the Failure.—Adiabatic pore moisture accumulated at the toe of the dam makes the clay swell. The process of swelling and consequent growth of moisture films gradually relieves original cohesion ($c = 0$ in Eq. 1). At the same time the growing value of the pore-moisture pressure ("neutral stress," u) gradually decreases the second member at the right side of Eq. 1. As soon as the values of u and p are balanced, the clay mass under the toes of the dam becomes an exceedingly soft plastic mass (shearing strength $s = 0$) under pressure. At this stage it breaks through the earth mass to reach the earth surface; and, in this connection, it must overcome what may be called "passive pressure" or "passive resistance" of the earth mass outside the dam. In other words, a shearing surface (failure line), to the right of the toe (Fig. 13), is formed, and a part of the clay material is thrown out to form the "upheaval" (or "mud wave"). As soon as this quasi-liquid mass reaches the earth surface, its pore pressure is lost (in the same way as pressure in the water running from a faucet). The clay gradually hardens; and sometimes one is able to walk on these "mud waves" a few days after failure.

The shape of the failure line (shearing surface) close to the middle of the dam may be circular in a general case (Fig. 13), but it is very possible that, under the given geological conditions, it is much steeper (Fig. 5). In the opinion of the writer (which he does not advance as a definite one, however), the separation of the wedge from the remainder of the dam in the given case is due mostly to tensile stresses.

All that has been said in this discussion about the character of the failure is nothing more than a proof and clarification of an interpretation presented by Messrs. Fields and Wells (see heading, "Analyses of Failure")—"the failure

* * * was caused by the building up of pore pressure in the clay stratum near the toes of the structure to values equal to or greater than the overburden pressure at that point." The writer agrees completely with this clear-cut statement since he considers it accurate enough for all practical purposes.

Summary.—Considerable work has been done in this field both in the United States and in England. The writer believes that research along these lines should be continued and that, in this connection, more attention should be paid to quantitative studies of soil permeability and drainage conditions under a dam. This remark is not intended to detract anything from the value of the papers discussed. In the last sentence of their paper, Messrs. Fields and Wells hope that "* * * the Pendleton experiences will assist, in some measure, the future studies of this most difficult question." The writer shares this hope and wishes to extend this statement to the British counterpart of the Pendleton Levee.[6]

AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

DISCUSSIONS

CHARACTERISTICS OF HEAVY RAINFALL IN NEW MEXICO AND ARIZONA

Discussion

BY MESSRS. L. L. HARROLD AND A. J. DICKSON, AND JAMES GIRAND

L. L. HARROLD,[10] ASSOC. M. AM. SOC. C. E., AND A. J. DICKSON,[11] ESQ.[11a]—The data presented by the author will be very useful to engineers engaged in hydraulic designs for the arid Southwest region. The writers have had occasion to make a concentrated tabulation and study of the type of rainfall reported in this paper.

The statement under the heading, "Storm Types," that "Valley areas usually receive the highest monthly precipitation in July, August, and September * * *" is assumed to mean that the precipitation in these months is usually higher than values for other months of the year. The records at the Navajo Experiment Station, Mexican Springs, N. Mex., verify this conclusion. Furthermore, the mountain gages at this research station usually record more monthly rainfall for these three months than for the other months of the year. This is contrary to the author's findings which probably are based on records from areas in Arizona and New Mexico where the winter snowfall is greater than at this research station. It is true, however, that the winter precipitation, December through March, at the higher elevations is significantly greater than that for the valley areas.

The data on areal extent of various quantities of summer rainfall (Table 1) are of great value even though rain gages were spaced rather far apart to delineate the limits of these areas adequately. The areal extent of summer storms over the watersheds of the Navajo Experiment Station (Fig. 6), 25 miles north of Gallup, N. Mex., cannot be well defined at all times although each of 83 rain gages represents an area of only 0.8 sq mile. Fig. 7 shows isohyetal lines for the total rainfall of the storm of August 6, 1941, a typical summer storm. It can be seen that the position of these isohyets would be entirely different if there were fewer gages so that each rain gage represented 10 sq miles. In areas where the gages are spaced 10 to 100 miles or more apart, it

NOTE.—This paper by Luna B. Leopold, Jun. Am. Soc. C. E., was published in February, 1943, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: April, 1943, by Lawrence Pratt, Assoc. M. Am. Soc. C. E.

[10] Hydr. Engr., SCS, U. S. Dept. of Agriculture, Washington, D. C.

[11] Asst. Hydr. Engr., SCS, U. S. Dept. of Agriculture, Mexican Springs, N. Mex.

[11a] Received by the Secretary May 11, 1943.

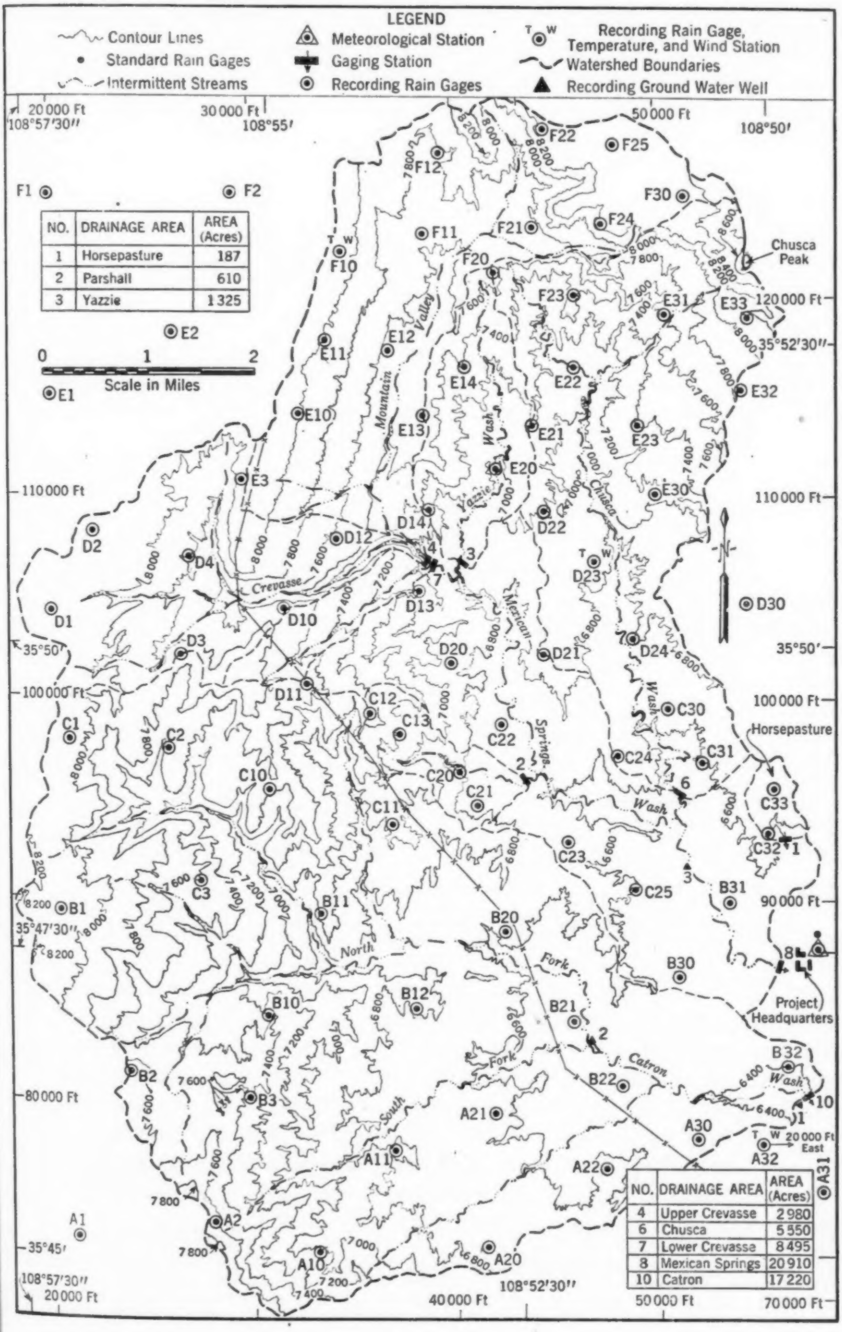
LEGEND
Contour Lines
Standard Rain Gages
Intermittent Streams
Meteorological Station
Gaging Station
Recording Rain Gages
Recording Rain Gage, Temperature, and Wind Station
Watershed Boundaries
Recording Ground Water Well
NO. DRAINAGE AREA AREA (Acres)
1 Horsepasture 187
2 Parshall 610
3 Yazzie 1325
Scale in Miles
NO. DRAINAGE AREA AREA (Acres)
4 Upper Crevasse 2980
6 Chusca 5550
7 Lower Crevasse 8495
8 Mexican Springs 20910
10 Catron 17220
Chusca Peak
Horsepasture
Project Headquarters
Mountain Valley
Crevasse
Yazzie Wash
Chusca Wash
Mexican Springs Wash
North Fork
South Fork
Catron Wash
108°57'30"
108°55'
108°52'30"
108°50'
35°52'30"
35°50'
35°47'30"
35°45'

Fig. 6

is no wonder that so little is known of the occurrence and distribution of summer storms. It is believed that on the Navajo Experiment Station there are more recording rain gages per unit than on any other area of the same size or larger in this region. These gages are operated in connection with runoff stations for the purpose of studying rainfall-runoff relations, land use, and water spreading. A description of some summer storms for 1941 on this experimental area is given elsewhere.[12,13]

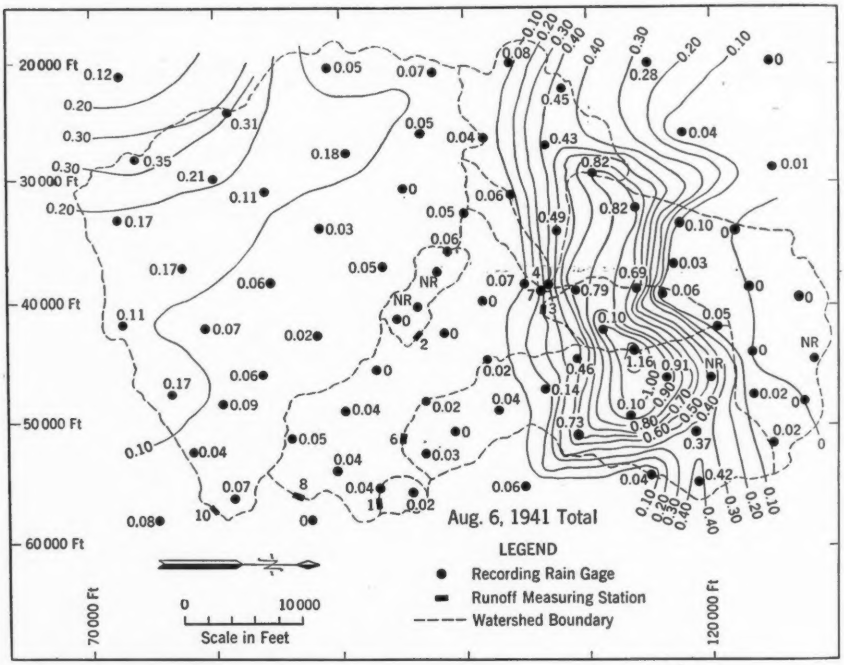

FIG. 7.—ISOHYETAL MAP OF TOTAL RAINFALL FOR STORM OF AUGUST 6, 1941, NAVAJO EXPERIMENT STATION, MEXICAN SPRINGS, N. MEX.

The records at this station confirm the author's statement (see heading, "Storm Types") that "The effect of topographic relief is relatively small * * *." They do not, however, tend to confirm his statement under the heading, "Areal Pattern of Summer Storms," that "* * * in any one summer storm the center of greatest precipitation usually is near the base of the mountain slope." On the Navajo Experiment Station area, elevations range from 6,200 to 8,800 ft, and, during 1941, six storms had thirteen different centers of greatest precipitation (Table 4). Eight of the thirteen centers were at elevations of from 7,000 to 8,000 ft and three were below and two above this range.

[12] "Agriculture, Soils, Geology, Topography, and 1941 Data, Navajo Conservation Experiment Station, Mexican Springs, N. Mex.," *Hydrologic Bulletin No. 6*, U.S.D.A. (publication pending).

[13] "Thunderstorms and Runoff at High Elevations in Northwestern New Mexico," by L. L. Harrold, *Transactions*, Am. Geophysical Union, 1943 (publication pending).

The author's conclusion (see heading, "Isopluvial Maps") "* * * that stations above El. 7500 experience relatively few high rainfalls" may be influenced largely by the fact that there were but few gages above this elevation. At this station, six out of thirteen centers of greatest precipitation were above El. 7500 (see Table 4).

TABLE 4.—CENTERS OF GREATEST PRECIPITATION FOR STORMS ON NAVAJO EXPERIMENT STATION, 1941

Date (1)	Rainfall station[a] (2)	Rainfall amount (in.) (3)	Elevation (ft) (4)	Date (1)	Rainfall station[a] (2)	Rainfall amount (in.) (3)	Elevation (ft) (4)
July 19	C32	0.87	6,620	August 8	B32	1.57	6,400
	D4	0.56	7,730		A22	1.39	6.710
	D13	0.56	7,300		D10	1.12	7,740
July 23	D1	2.40	8,080		E33	0.96	8,150
	E13	1.73	7,540		F21	0.80	7,740
August 6	E21	1.16	7,200	August 15	D22	1.05	7,020
				October 13	E22	1.44	7,060

[a] See Fig. 6.

The maximum point of rainfall for one day recorded on the experimental watershed during 1941 was 2.40 in. at gage D1 (Fig. 6). This corresponds to a storm of about a 10-yr to 15-yr recurrence according to the Tohatchi record in Table 3. Tohatchi is 7 miles northeast of the experimental watershed. According to the scale of the author's data in Table 3 for Tohatchi, the experimental watershed recorded three storms in 1941 having a 10-yr to 15-yr recurrence and two storms having a 5-yr recurrence. Some day it will be possible to give recurrence intervals for time-area-depth curves. Until such time engineers will have to be content with recurrence intervals for point rainfall records, which are none too satisfactory.

The author's statement that (see heading, "Isopluvial Maps") "* * * any high-intensity, individual storm rarely is recorded at more than one station" is obviously dependent on the spacing of rain gages. At this experiment station, where the gages are spaced about 1 mile apart, the following amounts, in inches, for the storm of July 23, 1941, were recorded at different gages: (2.40), (2.05), (2.02), (1.82), 1.73, 1.50, 1.39, and 1.07, in which values in parentheses belong to the same storm center.

This discussion is presented to emphasize the need for additional information on the behavior of summer storms of high intensity. As these storms are extremely erratic in time and areal distribution, the need can only be met by a rather concentrated network of recording rain gages operated over a long period. In order to give adequate areal coverage for deriving time-area-depth curves, it is estimated that the gages should be spaced 1 or 2 miles apart and the network should extend over roughly 250 sq miles.

JAMES GIRAND,[14] ASSOC. M. AM. SOC. C. E.[14a]—The utilization of meteorologic data in the design of engineering projects always has been surrounded

[14] Cons. Engr., Phoenix, Ariz.

[14a] Received by the Secretary May 17, 1943.

with uncertainties. The engineer approaches his problem with the conviction that some definite correlation between the rainfall records and the runoff data does exist. To establish a proof that will satisfy design requirements is another matter. It is apparent that any relations that do exist are highly complex, requiring the greatest perseverance to discover.

The author was fortunate in having available to him a relief project furnishing the large number of man-hours necessary to make a detailed study of a vast amount of rainfall data. This man power is seldom available to the designing engineer. The publication of the results of these studies is doubly valuable in that it opens a vista of the possibilities of further study and also develops definite information as to what continuing data should be collected. As the science of analyzing climatological data progresses, it should be possible to induce the Weather Bureau to collect its records in the most useful form. Eventually a card system might be devised so that these studies could be made with tabulating machines instead of such great expenditures in man-hours.

The interpretation of weather data is essentially a laborious process requiring a great amount of time and energy; but before this process can be set in motion, it is necessary to determine the basis upon which the analysis is to be made, the result to be sought, and the method of assembling and analyzing the recorded data. The author has not given a sufficiently complete exposition of the various assumptions and methods involved for others to apply his methods.

Weather data are basically erratic and unreliable. The use of short-term or broken weather records is extremely hazardous. However, meteorologic observations can be valuable when carefully analyzed so that the weight of numbers will offset the individual errors. This effect of erratic records is quite apparent in Table 3 under stations 206, 207, and 209, for Granite Reef Dam, Casa Grande, and Florence, respectively. These stations are about 25 miles apart and are similarly situated in regard to topographic features. However, Table 3 indicates that the one-day rainfall that probably will be reached or exceeded once in a hundred years is 2.50, 6.00, and 2.09 in., respectively. Obviously such wide variation could not be expected at three similar stations so closely grouped. This divergence is accounted for possibly by erratic records. Further examination shows that the author predicted 5-yr storms for the same stations to be 1.64, 1.63, and 1.72 in., respectively. These latter values appear reasonable.

The effect of elevation on rainfall in the Southwest is a field that deserves further study. The author touches on this by stating "rainfall * * * tends to increase with elevation," but he fails to investigate further. The writer's conclusion, based on numerous studies in Arizona, is that the rainfall is almost exactly proportional to the elevation, as shown in Fig. 8. It is surprising how close a relation is shown, particularly in the long-record stations. These studies seem to indicate that topographic influences are not as important as might be concluded from limited observations. If the relation between elevation and rainfall were introduced into the author's studies, more consistent and satisfactory results might be obtained.

The correlation of observation stations and elimination of erratic data should be the first steps in any rainfall-runoff study. Short-term or broken records should be correlated to that of the longest-term record available. When such a correlation is made, the probability curves of all similar stations will have the same shape and slope. The final results will then be consistent and will represent the nearest approach to an accurate solution.

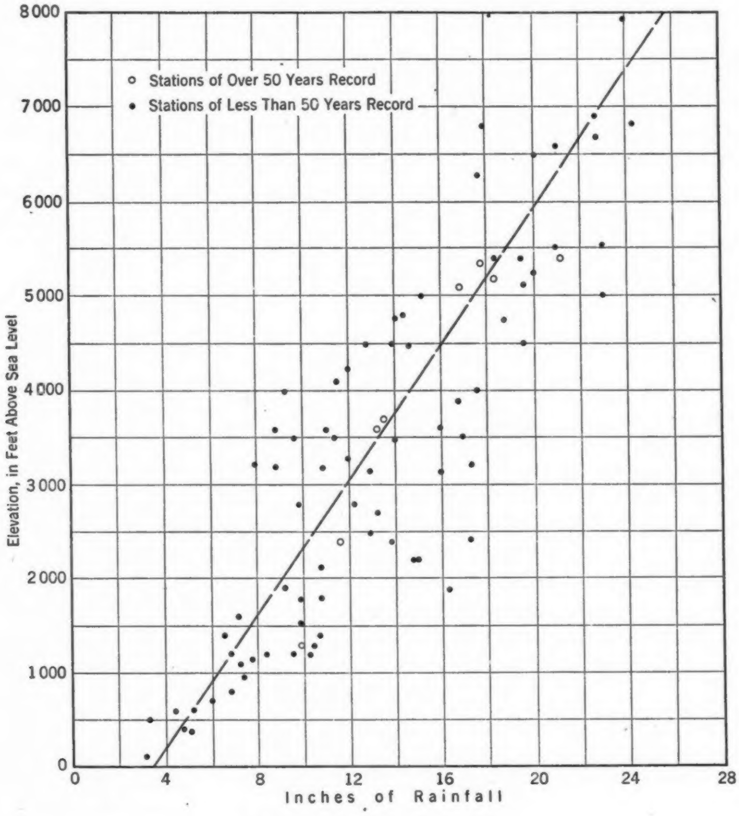

FIG. 8.—RAINFALL IN ARIZONA

In many cases it may be desirable to go beyond the terms of available records by some other method, such as the tree-ring technique of A. E. Douglass,[15] as applied by John Girand,[16] Assoc. M. Am. Soc. C. E. By such devices it is possible to ascertain whether the available records represent normal conditions, or periods of excessive drought or flood. Accurate predictions must be based on a complete knowledge of whether the data extrapolated

[15] "Climatic Cycles and Tree Growth," by A. E. Douglass, Carnegie Institution, Washington, D. C., 1928.

[16] "Water Supply on Upper Salt River, Arizona," by John Girand, *Transactions*, Am. Soc. C. E., Vol. 106 (1941), p. 398.

represent normal, wet, or dry years. The tree-ring technique has demonstrated the periodic recurrence of an excessive drought in Arizona at 300-yr intervals. This is highly important to the irrigation or water supply engineer. The presence of long-term periodic floods has not been proved adequately. For flood studies it is possible that existing rainfall records, some of which exceed 70 years, may be adequate as a base from which to predict future floods. The use of any shorter base is certain to lead to erroneous results.

Another valuable field of research would be to amplify the results shown in Table 1. The author's work and results, as set forth in Fig. 5, are for small watersheds. For the design of structures other than the smallest, it is essential that some relationship be found in terms of watershed area. The use of these figures for watersheds of even limited extent would lead to astronomical runoffs.

The author has made a substantial contribution to rainfall studies and it is hoped that he will be able to continue this work after he returns from duty with the armed forces. It is also to be hoped that, in the future, through various governmental agencies, the man power will be made available to the engineering profession for detailed studies such as this.

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DISCUSSIONS

CONFORMITY BETWEEN MODEL AND PROTOTYPE

A SYMPOSIUM

Discussion

BY MESSRS. GILBERT H. DUNSTAN, ROBERT F. KREISS, J. H. DOUMA, AND MARVIN J. WEBSTER

GILBERT H. DUNSTAN,[31] ASSOC. M. AM. SOC. C. E.[31a]—The various papers of this Symposium provide a valuable contribution to the literature of model testing. Time limitations in the teaching of hydraulics in most colleges, however, prevent more than a passing mention of the subject, except in advanced courses. Nevertheless, the writer believes that it is desirable to introduce model testing to students taking hydraulic laboratory work, and to call their attention to the various papers that have been published.

Since the value of a model depends largely upon the use that can be made of the results obtained from it, a practical demonstration will do much to convince students of the importance, validity, and limitations of model studies. This may not always be possible in a college laboratory, although the following arrangement has provided one solution at low cost.

Several years ago a standard 1-ft Parshall flume was constructed in the Hydraulics Laboratory at the University of Alabama, at University. It was made of timber, lined with sheet metal. The water supply available does not permit the calibration of the flume at rates of flow greater than about 1.5 cu ft per sec. It would be desirable to have at least twice this amount for the highest point on the calibration curve for this flume.

Subsequently, a small sheet metal flume was made with a 2-in. throat, or one sixth the prototype size. This can be calibrated over a wide range of flows. Students calibrate both the prototype and model flume. From the model data and the geometric scale ratio, the head for the prototype is obtained,

NOTE.—This Symposium was published in October, 1942, *Proceedings*. Discussion on this Symposium has appeared in *Proceedings*, as follows: December, 1942, by A. E. Niederhoff, Assoc. M. Am. Soc. C. E.; January, 1943, by Messrs. C. I. Grimm, and Joe W. Johnson; February, 1943, by V. L. Streeter, Assoc. M. Am. Soc. C. E.; March, 1943, by Glen N. Cox, M. Am. Soc. C. E.; April, 1943, by Messrs. Graham Walton, H. A. Einstein, K. G. Tower, R. J. Pafford, Jr., Edward H. Schulz, and F. T. Mavis; and May, 1943, by Messrs. D. C. McConaughy, L. Standish Hall, A. J. Gilardi, Fred W. Blaisdell, and A. R. Thomas.

[31] Associate Prof., San. Eng., Univ. of Alabama, University, Ala.

[31a] Received by the Secretary April 28, 1943.

and the prototype discharge is obtained from the model discharge by multiplying by $(L_r)^{2.5}$. The measured prototype data are plotted on a graph sheet, after which the prototype values computed from the model points are plotted.

Results obtained to date have shown good conformity, some students drawing only a single line to represent all points, whereas others represent the results by parallel lines, close together. In general, one or more model points, for the lowest flows, will be completely out of line. Computations show that, in the actual model test, the flow for these cases was laminar, whereas in the prototype it would be turbulent. This study, therefore, has been of value in demonstrating to students that a model may be used to predict prototype performance, provided that model laws and limitations are applied properly.

ROBERT F. KREISS,[32] JUN. AM. SOC. C. E.[32a]—The data from model and prototype on the locks discussed in this Symposium not only offer model-prototype comparisons but are useful to designers of navigation locks. The writer regrets the brief mention of dynamic head and overtravel and the omission of any description of apparatus for testing prototype locks.

The difficulty of comparing culvert systems in which port designs differ is appreciated. Lock coefficients vary not only with different port designs but with aggregate port area per culvert. Differences in coefficients for model and prototype are not unexpected for the first locks and the third locks (see Table 3), inasmuch as, in the first locks, the throat area of most of the prototype ports is symmetrically larger than in the model and, in the third locks, the area of the lock chamber end of each prototype port is larger. Coefficients are in good agreement in the single comparison having geometric similitude of manifold systems.

The effect of difference in port design on discharge capacity is clarified to some extent by the realization that lock coefficients for a single port design vary with the ratio of cumulative port area to culvert area. A relationship has been reported (76)[32b] wherein the lock coefficient, determined either as described in the paper or as an average of ten short intervals of a lockage, varies linearly on a semilogarithmic graph with the area ratio in the range from 0.89 to 2.38. The relationship is

$$C_l = \frac{\beta}{10^{\mu x}} \qquad (31)$$

in which C_l is the lock coefficient based on the exit area of all ports, x is the ratio of cumulative port exit-end area to culvert area, and β and μ, equaling 1.0 and 0.15, respectively, are constants for a lock-filling operation. The lock coefficient decreases from 0.73 to 0.44 over the range of area ratio. Differentiation gives

$$\frac{dC_l}{C_l} = \mu \log_e 10\, dx \qquad (32)$$

This expression would be of use not only in evaluating changes in C_l resulting

[32] Omaha, Nebr.

[32a] Received by the Secretary April 30, 1943.

[32b] Numerals in parentheses, thus: (76), refer to corresponding items in the Bibliography, which appears as the last unit of the Symposium, and at the end of discussion in this issue.

from small changes in area ratio for a given port design but also in approximating changes in C_l resulting from minor changes in port design if, by additional tests, the μ-values for each new design were found not to depart greatly from 0.15.

Eq. 31 is useful in a second and more important analysis. The lock coefficient, when computed from test data of short time intervals following the culvert valve opening, substantially eliminates the dynamic head effect. Consequently, Eq. 31 can be introduced into a steady flow discharge equation, as follows:

$$Q = \frac{\beta}{10^{\mu x}} A_p \sqrt{2 g h} \qquad (33)$$

in which A_p is the summation of port area. From Eq. 33 it can be shown that the area ratio, at which percentage changes in either port area or culvert area are equally effective in changing the discharge capacity, is

$$x = \frac{1}{2 \mu \log_e 10} \qquad (34)$$

From Eq. 34, note that the area ratio is 1.45 when μ is 0.15. A percentage change in culvert area becomes increasingly more effective on discharge capacity as the area ratio progressively increases above 1.45. For example, the culvert area is twice as effective as the port area when the area ratio is 1.93. With area ratios less than 1.45, the culvert is less effective than the ports.

The venturi-type port design, to which Eq. 31 applies, has a throat area 51.8% as large as the exit-end area. The ratio of total throat area to culvert area is 0.75 when the area ratio x is 1.45. The area ratio is 1.93 when the total throat area is the same as the culvert area.

Eq. 34, which is applicable to manifold systems satisfying Eq. 31, is sufficient proof that the minimum flow area in a manifold system need not be the major control area for discharge. Furthermore, the shifting from port area to culvert area, as the minimum flow area passes from ports to culvert or at any area ratio, is not advisable in computing coefficients, since each area—culvert, total port throat, or total port exit (for flared ports)—provides some degree of control on discharge. Coefficients throughout the range of area ratio should be expressed in terms of one area, say that of the culvert, with a qualifying expression regarding area ratio, as $C_l = 0.62$ at $x = 1.50$.

The development of expressions comparable to Eq. 31 is needed. The exponential function is suitable probably for venturi-type ports in a manifold system but may not be applicable for square-cornered ports. For systems satisfying Eq. 31, the constants β and μ are of greater interest than the variable C_l. Their determination is difficult in prototype lock tests because of the requirement that the area ratio be changed. In model tests their measurement is made more easily.

The writer agrees with the Symposium authors that inclusion of dynamic head effects in discharge coefficients is satisfactory in model-prototype comparisons. For design purposes coefficients eliminating dynamic head effects are preferred.

J. H. Douma,[33] Jun. Am. Soc. C. E.[33a]—Interesting results and useful hydraulic data on problems in the field of model-prototype conformity that are not clearly understood generally by hydraulic engineers are presented in the papers of this Symposium. The first paper, entitled "Hydraulic Structures," is especially valuable to future conformity studies because of its comprehensive outline of the factors that make satisfactory model-prototype comparisons difficult. Future investigators will do well to use this outline as a guide in developing testing techniques and in eliminating experimental errors.

The results of the qualitative model-prototype comparison, described by Messrs. Warnock and Dewey, together with results of other similar comparisons appear to support the conclusion that qualitative model studies, conducted under the present-day technique, are reliable in predicting many prototype conditions with a satisfactory degree of accuracy. Quantitative comparisons, however, have frequently shown wide differences in model and prototype results. The explanation undoubtedly is that satisfactory quantitative comparisons require more exact reproduction of the hydraulic phenomena involved to make possible accurate predictions of pressures, velocities, water-surface profiles, depths of scour, and discharges.

Discrepancies in quantitative model-prototype comparisons often have been explained as due to the lack of strict dynamic similarity, when usually other factors are the chief sources of error. Frequently, the statement is made that Reynolds' criterion has not been satisfied; and that, therefore, prototype quantities are not predicted accurately by the model. Messrs. Warnock and Dewey use this reason to explain the differences in model and prototype pressures and discharges of the 102-in. outlet conduit.

Many large-scale models do not require strict compliance with Reynolds' law. Under conditions of fully developed turbulent flow in a model, or prototype, the friction coefficient is constant for any one roughness value and independent of viscosity. Therefore, model-prototype comparisons do not require compliance with Reynolds' criterion when the model scale and roughness are selected so that the friction coefficient in the model and prototype is equal and so that fully developed turbulent flow occurs in both model and prototype. A brief review of the laws of similitude will assist in clarifying the limitations of model-prototype comparisons.

For a model to be a true representation of its prototype in every sense, both geometric and dynamic similarity must exist at the same time. Geometric similarity of the solid boundaries, including the roughness, sets the conditions for geometric similarity of the flow stream lines. Dynamic similarity of the exerting forces, and thus similarity of the fluid properties, sets the conditions for similarity of the flow characteristics. Geometric similarity is obtained by fixing all model dimensions in some direct scale ratio to corresponding prototype dimensions. Strict dynamic similarity, however, is not obtained so easily as every dimensionless parameter referring to the flow characteristics in the model must have the same numerical magnitude as the corresponding parameter referring to the prototype. In other words, the flow characteristics

[33] Hydr. Engr., U. S. Engr. Office, Los Angeles, Calif.

[33a] Received by the Secretary May 3, 1943.

will be similar if the fluid properties are such that the gravitational, viscous, capillary, and elastic forces bear the required relationship of model to prototype (57*a*).

The basic dimensionless parameters pertaining to the action of weight, viscosity, surface tension, and elasticity are known, respectively, as the Froude, Reynolds, Weber, and Cauchy numbers:

$$\mathbf{F} = \frac{\rho V^2}{\gamma L}; \mathbf{R} = \frac{V L}{\mu \rho}; \mathbf{W} = \frac{\rho V^2 L}{\sigma}; \mathbf{C} = \frac{\rho V^2}{E}$$

in which $\mathbf{F}$ = Froude's number; $\mathbf{R}$ = Reynolds' number; $\mathbf{W}$ = Weber's number; $\mathbf{C}$ = Cauchy's number; V = mean velocity; L = a length (diameter for pipes and hydraulic radius for open channels); γ = specific gravity; ρ = density; μ = viscosity; σ = surface tension; and E = elastic modulus. The magnitude of these numbers, assuming geometric similarity, must be equal in model and prototype to attain complete similarity.

When more than two of the force properties of a fluid appreciably influence some phenomenon of flow it is impossible to obtain significant quantitative observations from a model. Therefore, it is necessary to eliminate at least all but two of the force properties in order to make a model test.

For all practical purposes, elastic forces may be ignored in the usual model study, since both model and prototype are assumed to be rigid and the elastic properties of the model and prototype fluids are identical when the same fluid occurs in the model and prototype. Also, capillary forces need not be considered unless the model is extremely small and surface tension forces are appreciable.

The Froude and Reynolds criteria for similitude remain to be considered. Although both criteria cannot be satisfied when the same fluid (water) occurs in the model and prototype, it is possible to select a different model fluid with kinematic viscosity such as to satisfy both criteria at the same time. In practice, however, it is desirable to use water as the model fluid. Satisfactory model tests can be made with water when either the viscous or the gravitational forces are predominant and all other forces are of secondary importance. The proper law for similitude would be that corresponding to the predominant force.

A criterion from which to determine when Reynolds' law must be satisfied can be developed from the experiments of J. Nikuradse (59) on pipes. In his experiments the relationship between the friction factor f, as defined by the Weisback formula

$$h_f = \frac{f L V^2}{2 g D} \qquad (35)$$

and the Reynolds number was determined for flow in pipes. Typical plots of the relationship are shown in Fig. 80. It is noted that for laminar flow a small change in Reynolds' number results in a large change in the friction factor, whereas for large Reynolds numbers with fully developed turbulence and for a constant boundary roughness there is no appreciable change in the friction factor for a wide range in Reynolds' number. Thus, when laminar flow exists

in either the model or the prototype, Reynolds' law must be satisfied. On the other hand, when fully developed turbulent flow exists in both the model and prototype and the model scale and roughness are such that the friction coefficients are nearly equal, then the viscous forces would have a minor influence on similitude and Reynolds' law need not be satisfied.

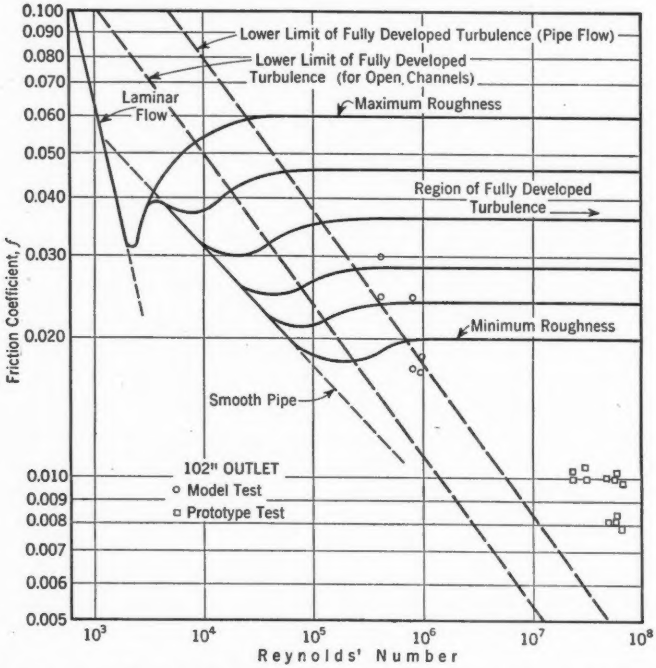


FIG. 80.—REYNOLDS' NUMBER VS. FRICTION COEFFICIENT—TYPICAL CURVES FOR PIPE FLOW

Nikuradse's experiments on pipes revealed that the lower limit of Reynolds' number for which fully developed turbulent flow occurred varies with the boundary roughness. Fully developed turbulence existed for all of the roughness values tested when the Reynolds number exceeded one million. For the maximum roughness tested, fully developed turbulence existed for a Reynolds number of 13,000.

Based on the results of Nikuradse's experiments, a straight line, representing the lower limit of Reynolds' number at which fully developed turbulence occurs in pipes, was drawn in Fig. 80. For purposes of discussion, it is assumed that the lower-limit line extends to an f value of 0.005 as shown in Fig. 80. Additional tests on larger and smoother pipes than those tested by Nikuradse would be required to verify the assumed extension.

By plotting, in Fig. 80, the model and prototype test results for the 102-in. outlet conduits reported in the paper, it is shown that fully developed turbulence existed for all of the model and prototype tests, except for two of the model tests which plot in the transition region from laminar flow to fully developed turbulent flow. Furthermore, the values of the friction coefficients in the model tests were two to three times those in the prototype. Under these conditions,

viscous forces should not be neglected since they affect similitude appreciably. Better agreement between model and prototype tests for the 102-in. outlet conduits probably would have been obtained had the model scale been larger and the model surfaces smoother.

Based on the satisfactory model-prototype comparisons for the outlet conduits reported in the paper, it appears that, for purposes of design, strict compliance with Reynolds' law is not required. Strict compliance is probably required, however, when a discharge rating curve is to be predicted accurately by a model. Study of additional model-prototype comparisons will be required to determine the limitations as to when and to what degree Reynolds' law need be satisfied.

For comparison with the lower limits of Reynolds' numbers for fully developed turbulence in pipes a similar curve for open channels is shown dotted in Fig. 80. Until more reliable limits are established, these limiting lines may be used by model designers to check model requirements with regard to Reynolds' law. For example, if a proposed model is so small that test points would plot to the left of the appropriate limiting line, then Reynolds' law should be considered.

With reference to open channels, the laws pertaining to viscous forces are not well defined, and it is necessary to base an analysis of open-channel flow on the established laws for pipe flow. The Reynolds number for open-channel flow is determined by the same expression as that for pipe flow except that the hydraulic radius is used as the linear dimension instead of the pipe diameter. Therefore, Reynolds' numbers for flow in open channels, under conditions of equal velocities and hydraulic radii, will be one fourth the Reynolds numbers for pipes, since the hydraulic radius of a pipe is one fourth the pipe diameter. Accordingly, the limiting line for fully developed turbulence in open channels was drawn with values of Reynolds' numbers equal to one fourth the limiting values for pipes.

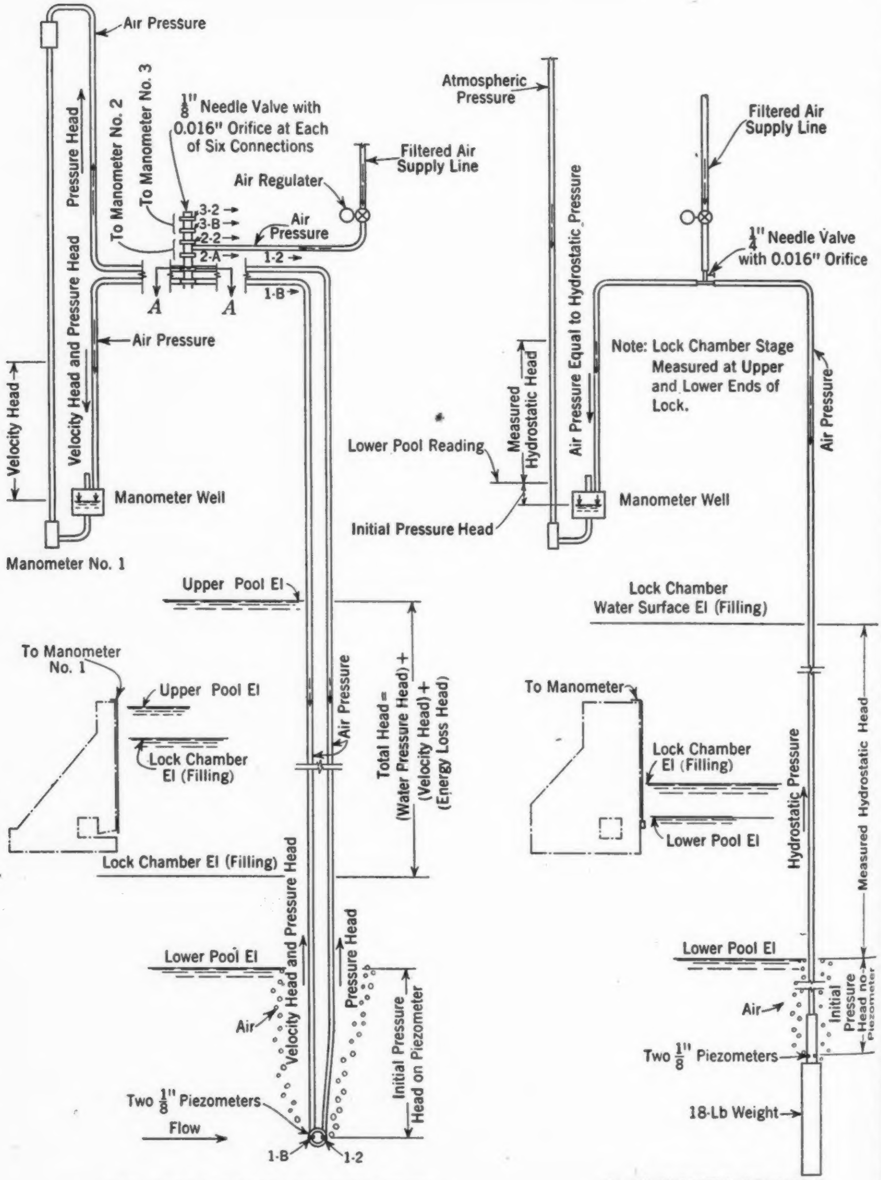
MARVIN J. WEBSTER,[34] ASSOC. M. AM. SOC. C. E.[34a]—The design of apparatus for correlation tests between models and their prototypes often presents unusual difficulties. This was especially true of the apparatus used in the prototype tests discussed in the paper by Messrs. Nelson and Hartigan, and it is regretted that their paper did not include a brief description of the apparatus.

The authors have based their comparisons on results of stage tests and port velocity tests. Stages in a lock chamber during filling and emptying operations can be measured with apparatus of relatively simple design. However, accurate measurement of velocities of the magnitude estimated and under the conditions prevailing in lock chambers with extremely high lifts was without known precedent. Although other methods of discharge measurement were considered, the selection was reduced to the current meter and pitot tube.

In their discussion on port velocity tests on the first lock, the authors state that during a filling operation discharge through ports 1 and 3 was reversed, flowing from the lock chamber into the culvert. During the operation of the tests on some of the other locks, it was observed that the direction of flow

[34] St. Paul, Minn.

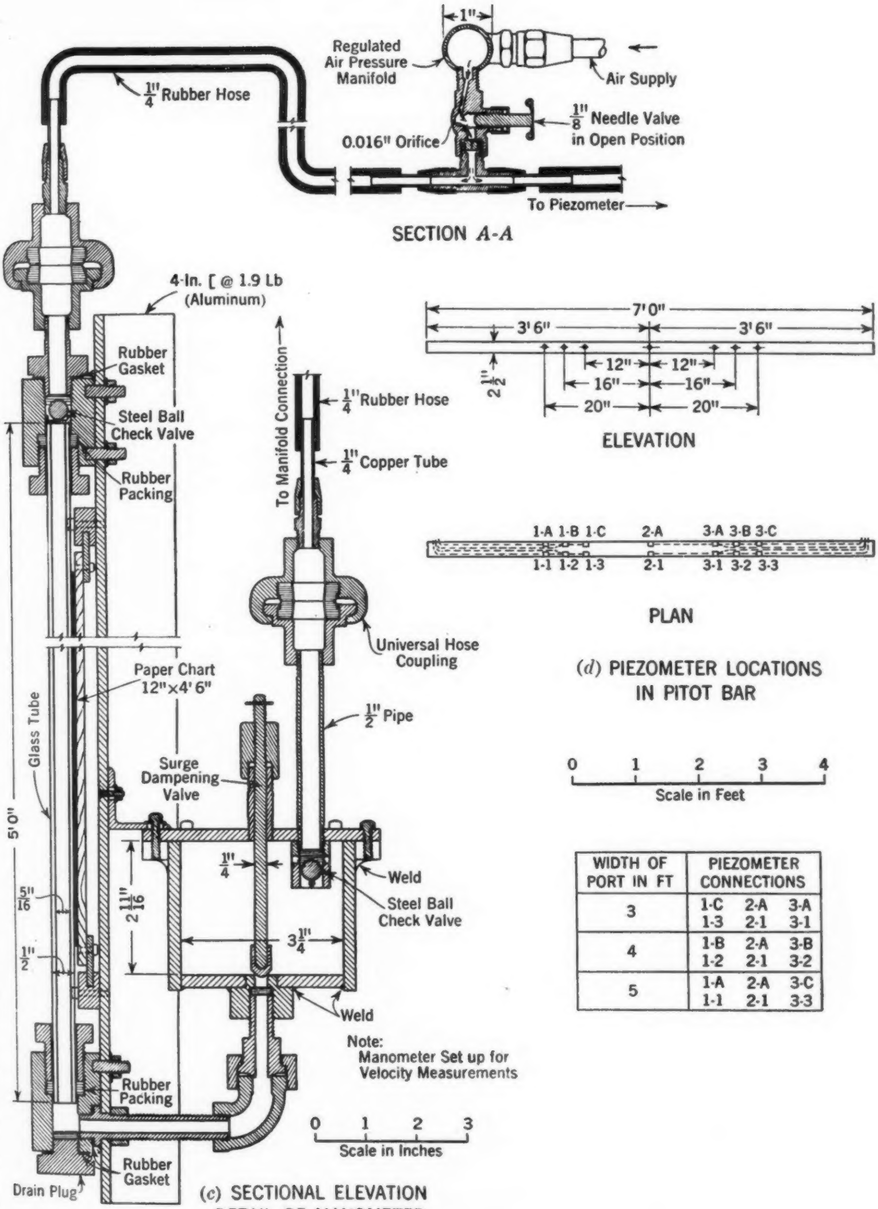
[34a] Received by the Secretary May 24, 1943.



(*a*) FOR PORT VELOCITY MEASUREMENTS

(*b*) PNEUMATIC SOUNDER FOR MEASUREMENT OF LOCK CHAMBER STAGE

FIG. 81.—DIAGRAMMATIC



(c) SECTIONAL ELEVATION DETAIL OF MANOMETER

(d) PIEZOMETER LOCATIONS IN PITOT BAR

WIDTH OF PORT IN FT	PIEZOMETER CONNECTIONS		
3	1-C 1-3	2-A 2-1	3-A 3-1
4	1-B 1-2	2-A 2-1	3-B 3-2
5	1-A 1-1	2-A 2-1	3-C 3-3

SKETCHES OF INSTRUMENTS

changed frequently and rapidly during the filling operation. This condition was anticipated and was one of the principal reasons why current meters were not adopted; namely, they would not respond quickly to velocities changing rapidly in both direction and intensity. Also, current meters would offer considerable restriction to the flow if velocities were measured simultaneously at several points in the port.

Because the distance from the point of measurement to the point of observation greatly exceeded the barometric head, the pitot tube could not be used in its usual form. Since compressed air was a suitable medium and since all locks were equipped with compressed air, it was selected as the transmitting medium.

Diagrammatic sketches of the instruments used are shown in Fig. 81. Velocity heads were observed on manometers placed on top of the lock wall. A manometer with connections for port velocity tests is shown in Fig. 81(*c*). Mercury was used in the manometers for the higher range of velocities; bromoform was used for consistently low velocities where deflection of a mercury column was insufficient for accurate readings.

The pitot bar (Fig. 81(*c*)) was equipped with piezometers for simultaneous readings at three points in the flow area. The bar offered a minimum of restriction to the flow through the lock-chamber ports and could be supported rigidly by an aluminum frame anchored to the lock-chamber wall above the lower pool water line.

The pitot bar was not calibrated before the tests were made. A coefficient of unity was used until all tests were completed. Then the coefficient of the bar for each lock was computed by correlating port velocity and lock-chamber stage measurements. The average coefficient of 0.84 was obtained from the data of all locks tested for both filling and emptying operations.

The manometers, with slight changes in connections and arrangement (Fig. 81(*b*)), were used for measuring stages in the lock chamber. If current meters had been used, one set of instruments would have been required for the port velocity tests and an entirely different set for the stage tests.

One major change in the apparatus was found necessary after the tests were started: Proper adjustment of the air supply could not be obtained with the $\frac{1}{8}$-in. needle valves. The difficulty was overcome by the installation of a 0.016-in. orifice in each connection between the air-pressure manifold and the individual manometers. This arrangement was very satisfactory and permitted considerable latitude in air pressure without affecting the readings observed on the manometers.

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AMERICAN SOCIETY OF CIVIL ENGINEERS
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DISCUSSIONS

EFFECT OF TURBULENCE ON SEDIMENTATION

Discussion

BY MESSRS. H. A. EINSTEIN, AND THOMAS R. CAMP

H. A. EINSTEIN,[6] ASSOC. M. AM. SOC. C. E.[6a]—Ostensibly resulting from the study of sedimentation problems in settling basins, this paper is especially interesting to the hydraulic engineer who is not familiar with this special problem. Artificial settling basins, as studied and applied by the sanitary engineer, usually are characterized by very small average flow velocities of the water. Professor Camp (2)[6b] showed that under certain circumstances an increase of turbulence in such basins will shorten the settling time if this additional mixing affects coagulation thus increasing, materially, the settling velocity of small particles. The hydraulic engineer studying sedimentation in natural streams finds those conditions only in rare cases. Usually the turbulent mixing in natural streams is sufficient to prevent the clay from settling independently of the stage of flocculation. In many special cases, however, where high sediment concentration and salt content favor flocculation, this effect may explain otherwise incomprehensible situations.

Most significant in Mr. Dobbins' paper is the emphasis given to what he calls "pickup"—the exchange of particles between bed and suspension. In another connection (17), the writer has showed the paramount importance of this same exchange of particles in the description of bed-load movement, and probably this same exchange or pickup will some day make it possible to calculate the rates of bed material carried in suspension without the use of any concentration measurements.

In his experiments Mr. Dobbins used, exclusively, "artificial turbulence," that is, turbulence created by a system of baffles rather than by friction along the sediment deposit. The intensity of the resulting turbulence is not measured, and, therefore, no comparison with conditions in a natural river is possible. As a check of the formulas, the experiments are excellent and prove

NOTE.—This paper by William E. Dobbins, Jun. Am. Soc. C. E., was published in February, 1943, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: April, 1943, by A. M. Gaudin, Esq.; and May, 1943, by John S. McNown, Jun. Am. Soc. C. E.

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[6a] Received by the Secretary April 5, 1943.

[6b] Numerals in parentheses, thus: (2), refer to corresponding items in the Bibliography (see Appendix 1) of the paper, and at the end of discussion in this issue.

the formulas to be correct although somewhat complicated for practical use. The exceptionally clear presentation of this intricate subject makes Mr. Dobbins' paper an outstanding contribution to the literature on sedimentation in general.

THOMAS R. CAMP,[7] M. AM. SOC. C. E.[7a]—A significant contribution to current knowledge of sedimentation principles, this paper demonstrates clearly that suspended sediment is not held in suspension in a turbulent stream; but rather that it is settling out continuously and is being thrown back continuously into the stream by scour from the bottom. The concentration of suspended matter at any depth in a stream at equilibrium is thus shown to depend primarily upon the rate of scour from the bottom. When the rate of scour is equal to the rate of settling out, equilibrium prevails at all levels. When the rate of scour exceeds the rate of settling out, the concentration is increasing at all depths. When the rate of scour is less than the rate of settling out, the stream is clearing. If scour is prevented, the stream will clear completely; but the time required for clearing depends upon the settling velocity of the particles, the depth, and the magnitude of the turbulence.

The equilibrium condition characterized by Eqs. 3 and 4 has been widely used in the study of suspended-load distribution. It has been applied also to the measurement of evaporation of moisture from land and water surfaces (18), and to the movement of sand dunes by wind (19). In the light of Mr. Dobbins' studies, it is evident that, in all three problems, the equilibrium condition is only approximated in nature. The suspended matter in a stream and the sand in the air will be at the equilibrium concentration only when the rate of pickup equals the rate of settling out. The moisture in the air will be at the equilibrium concentration only when the rate of transport upward is the same across all horizontal planes.

The studies described by Mr. Dobbins were undertaken as a part of a larger program aiming at a rational approach in the design of settling tanks. In a settling tank the rate of scour from the bottom is always less than the rate of settling out. In most types of settling tanks, there should be no scour from the bottom if the tank is functioning properly. The effect of turbulence is to retard the rate of clearing. The immediate objective of the author's studies was to obtain some estimate of the magnitude of this retarding effect.

Eq. 41 is an expression for the concentration at any height y and any time t for the one-dimensional case, the initial concentration being uniformly distributed and equal to c_o from top to bottom. This corresponds to a value of ∞ for the mixing coefficient ϵ at the entrance to a settling tank, followed immediately by a finite value of ϵ within the tank. It is convenient to assume a uniform distribution of suspended matter at the start in order to compare the removal with that obtained under similar conditions but in the absence of turbulence.

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[7a] Received by the Secretary April 14, 1943.

In the absence of turbulence and scour, starting with uniform distribution of suspended matter, the removal after any time t of all particles whose settling velocity w is less than w_o is

$$r = \frac{w}{w_o} \qquad (47)$$

in which $w_o = \frac{h}{t}$ = the "overflow rate." In the sanitary engineering field, the "overflow rate" is usually defined as the discharge per unit of surface area. It is also the settling velocity required for a particle to settle throughout the depth h in time t. Therefore all particles with settling velocities exceeding w_o will be 100% removed in time t.

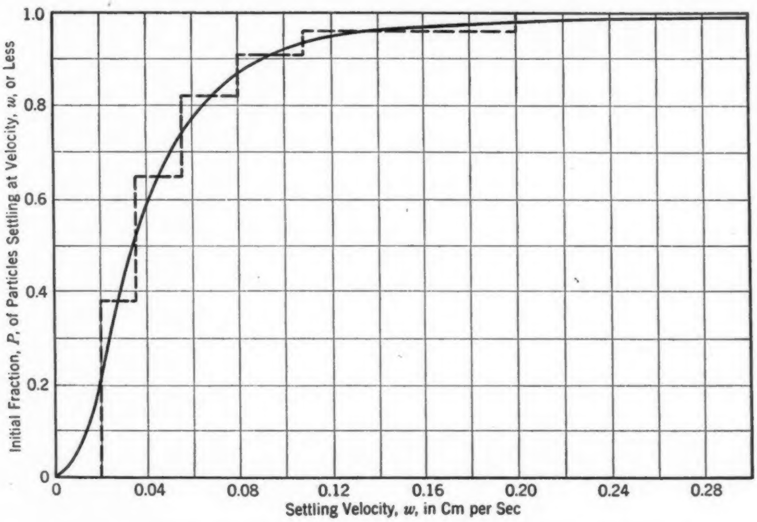

FIG. 14.—SETTLING VELOCITY ANALYSIS OF SUSPENSION OF DISCRETE PARTICLES

In the absence of scour, A is zero in Eq. 41. For this condition, Eq. 41 can be rewritten in the following dimensionless form:

$$\frac{c}{c_o} = \frac{w\,h}{2\,\epsilon}\,e^{i} \sum_{n=1}^{n=\infty} \frac{(h\,\alpha_n)^2\,H_n\,Y_n\,e^{-j}}{\left[\left(\frac{w\,h}{2\,\epsilon}\right)^2 + (h\,\alpha_n)^2 + \frac{2\,w\,h}{2\,\epsilon}\right]\left[\left(\frac{w\,h}{2\,\epsilon}\right)^2 + (h\,\alpha_n)^2\right]} \qquad (48)$$

in which, to simplify typography,

$$i = \frac{w\,h}{2\,\epsilon}\left(1 - \frac{y}{h}\right) \qquad (49a)$$

and

$$j = \left[\left(\frac{w\,h}{2\,\epsilon}\right)^2 + (h\;\alpha_n)^2\right] \frac{1}{\frac{2\,w\,h}{2\,\epsilon}} \frac{w}{w_o} \qquad (49b)$$

The relative concentration $\frac{c}{c_o}$ in Eq. 48 is a function of only three independent variables, $\frac{y}{h}$, $\frac{w\,h}{2\,\epsilon}$, and $\frac{w}{w_o}$, all of which are dimensionless.

The application of Eq. 48 to a suspension of particles of varying settling velocity requires that the analysis of the suspension in terms of settling velocity be known. Fig. 14 is a typical settling-velocity analysis of a suspension of discrete particles. In order to apply Eq. 48, the suspension should be fractionated as shown by the broken line. The equation is applied separately to each

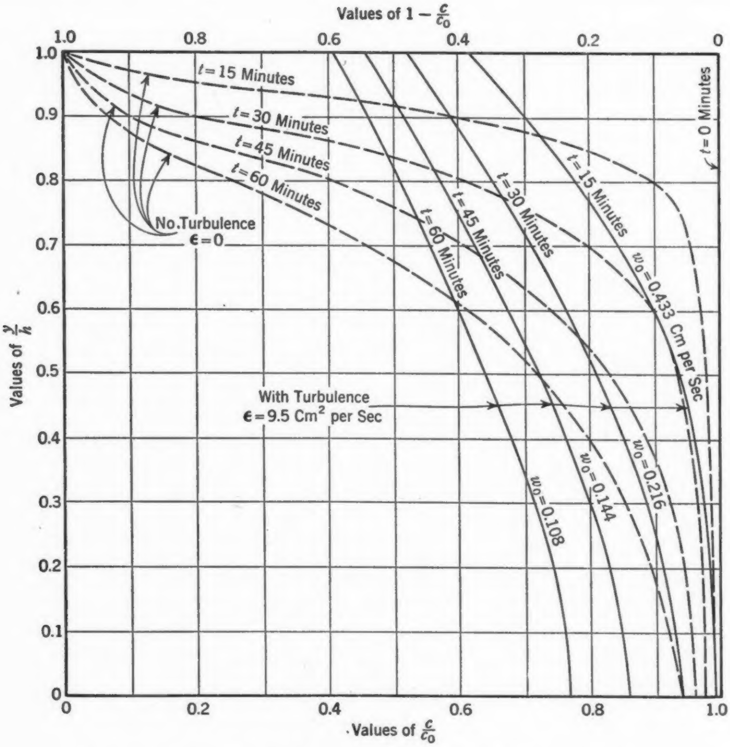

FIG. 15.—EFFECT OF TURBULENCE IN TANK 12.8 FT DEEP UPON SETTLING OF SUSPENSION OF FIG. 14

fraction, assuming that all particles in each fraction have the same settling velocity as shown; and the total relative concentration then is computed by adding the fractional values of $\frac{c}{c_o}$. Fig. 15 shows the results of such a computation. The corresponding values of $\frac{c}{c_o}$ in the absence of turbulence are shown by broken lines for comparison.

The average relative concentration throughout the depth h may be computed from the integral $\int_0^1 \frac{c}{c_o}\, d\left(\frac{y}{H}\right)$ in which the value of $\frac{c}{c_o}$ is taken from Eq. 48. This integral is evaluated readily. The removal from suspension by settling in time t is the difference between this integral and the initial concentration. Since the relative initial concentration must be taken at unity throughout the depth h, the relative removal with turbulence is

$$r = 1 - 8\left(\frac{w\,h}{2\,\epsilon}\right)^2 e^{(w\,h)/(2\,\epsilon)}$$
$$\times \sum_{n=1}^{n=\infty} \frac{(h\,\alpha_n)^2\,H_n\,e^{-i}}{\left[\left(\frac{w\,h}{2\,\epsilon}\right)^2 + (h\,\alpha_n)^2 + \frac{2\,w\,h}{2\,\epsilon}\right]\left[\left(\frac{w\,h}{2\,\epsilon}\right)^2 + (h\,\alpha_n)^2\right]^2} \qquad (50)$$

The term Y_n which appears in Eq. 48 has been eliminated in Eq. 50 by integration over the depth h. The relative removal in Eq. 50 is a function of only two independent variables, $\frac{w\,h}{2\,\epsilon}$ and $\frac{w}{w_o}$, both of which are dimensionless.

Numerical computations by means of Eqs. 48 and 50 are extremely tedious and cumbersome. Some problems of practical significance require fifteen or more terms for convergence of the series. Moreover, for such problems, the graphical evaluation of $h\,\alpha$ as illustrated by the author in Fig. 3 is not sufficiently accurate. It is convenient, therefore, to have a graph for solution of numerical problems; and the writer has prepared such a graph for Eq. 50. Fig. 16 is a dimensionless plot of r against $\frac{w\,h}{2\,\epsilon}$ for various values of $\frac{w}{w_o}$. The successive values of $h\,\alpha_n$ required for each computed point on this graph were determined to six decimal places by trial-and-error solutions of Eq. 31. Convergence of the series is rapid for low values of $\frac{w\,h}{2\,\epsilon}$, only one term being required for a value of 0.1. However, fourteen terms were required for $\frac{w\,h}{2\,\epsilon} = 30$, and no solution was practicable for $\frac{w\,h}{2\,\epsilon} = 100$.

The effect of turbulence in retarding settling for the one-dimensional case is apparent from Fig. 16. When turbulence is relatively great with a high value of ϵ, the value of $\frac{w\,h}{2\,\epsilon}$ is low and removal is reduced. For example, for particles that would be just 100% settled without turbulence $\left(\text{that is, } \frac{w}{w_o} = 1.0\right)$, the removal is only 64% if $\frac{w\,h}{2\,\epsilon} = 0.1$; but it is 72% if $\frac{w\,h}{2\,\epsilon} = 1.0$; and it is 94% if $\frac{w\,h}{2\,\epsilon} = 40$. The effect of turbulence on the settling out of particles is much less if the removal is less—that is, for low values of $\frac{w}{w_o}$.

Eqs. 48 and 50 and Fig. 16 are correct solutions for only the one-dimensional case. However, they may be used for two-dimensional flow in a stream

or settling tank to obtain approximate solutions. The following bold assumptions are required in the adaptation to two-dimensional flow:

1. The fluid velocity is the same at every point in the channel; and
2. The mixing coefficient ϵ has the same value at every point in the channel.

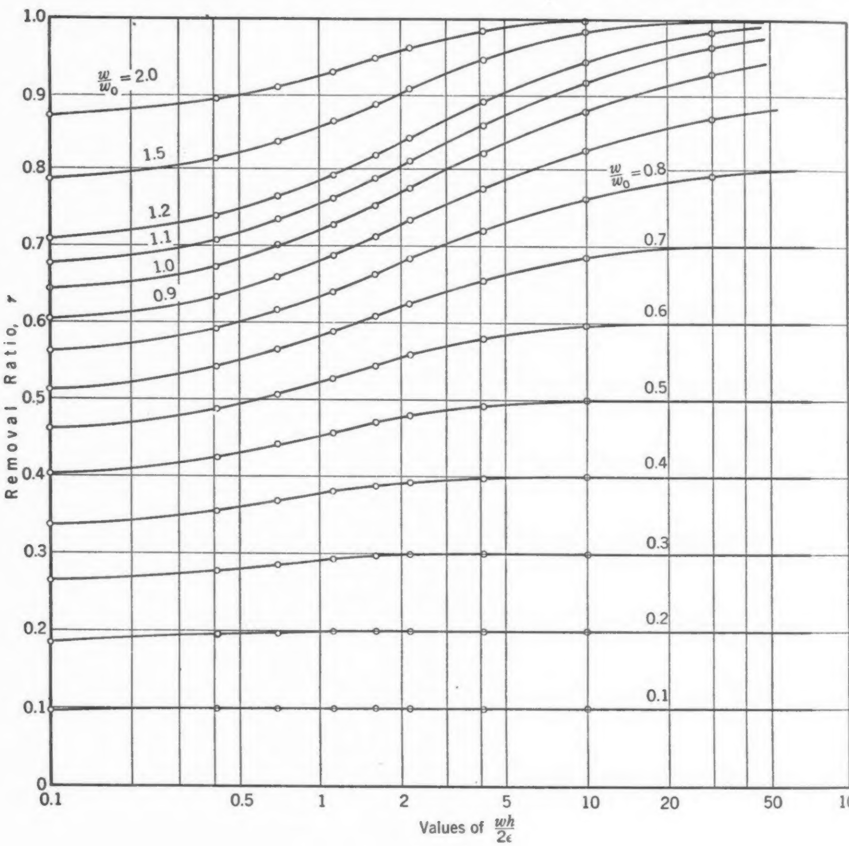

FIG. 16

By reference to Eq. 16*b*, it will be noted that the second assumption corresponds to a parabolic velocity distribution as illustrated by Fig. 2. Obviously, then, the two assumptions are contradictory; but they permit the use of Eqs. 17*b* and 17*c*, the only form of the general differential equation for which a solution has been obtained.

The value of ϵ at any height y for two-dimensional open-channel flow and the logarithmic velocity distribution is given by Eq. 11, which may be rewritten in dimensionless form as follows:

$$\frac{\epsilon}{h\,\bar{V}\sqrt{\frac{f}{8}}} = \kappa\,\frac{y}{h}\left(1 - \frac{y}{h}\right) \quad \text{.....(51)}$$

This value of ϵ and the corresponding velocity distribution are shown with broken lines in Fig. 17. The mean value of ϵ over the depth h is given by

$$\frac{\epsilon}{h\,\bar{V}\,\sqrt{\frac{f}{8}}} = \frac{\kappa}{6} = 0.0667 \qquad (52)$$

in which the value of κ is taken at 0.4. Eq. 52 is shown by the solid vertical line in Fig. 17. If this value of ϵ is assumed to obtain from top to bottom, the corresponding parabolic velocity distribution is as shown by the solid curve in Fig. 17.

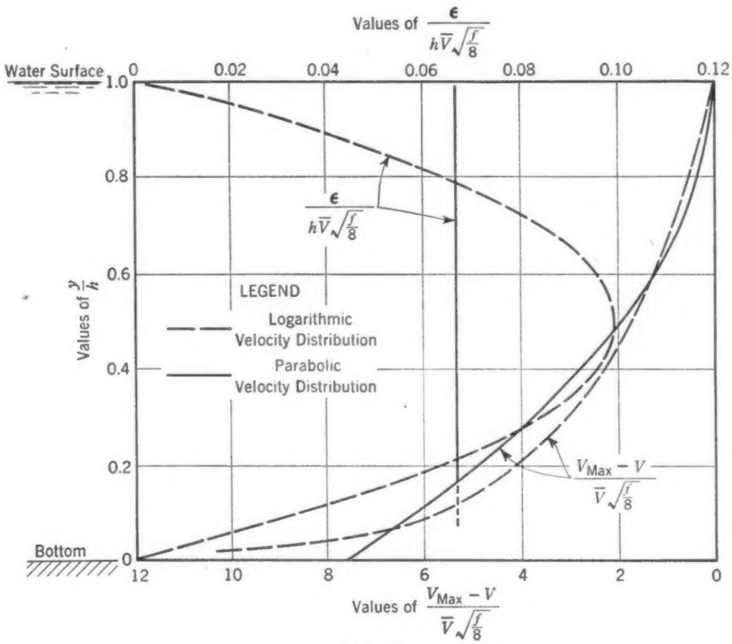

FIG. 17

The value of $\frac{w\,h}{2\,\epsilon}$ for two-dimensional open-channel flow may be expressed in terms of the mean velocity of the stream by means of Eq. 52 as follows:

$$\frac{w\,h}{2\,\epsilon} = \frac{3\,w}{\kappa\,\bar{V}\,\sqrt{\frac{f}{8}}} = 7.5\,\frac{w}{\bar{V}\,\sqrt{\frac{f}{8}}} \qquad (53a)$$

Also, since by similar triangles $\frac{w_o}{\bar{V}} = \frac{h}{L}$ (in which L is the length of travel at the mean velocity $\bar{V}$ in time t)—

$$\frac{w\,h}{2\,\epsilon} = \frac{3}{\kappa\sqrt{\frac{8}{f}}}\,\frac{h}{L}\,\frac{w}{w_o} = \frac{7.5}{\sqrt{\frac{f}{8}}}\,\frac{h}{L}\,\frac{w}{w_o} \qquad (53b)$$

As an example, let it be required to find the effect of turbulence on the settling of particles which would be just 70% removed $\left(\frac{w}{w_o} = 0.70\right)$ in the absence of turbulence in a 200-ft stretch of tank 10 ft deep $\left(\frac{h}{L} = 0.05\right)$ if $f = 0.032$. From Eq. 53*b*, $\frac{w\,h}{2\,\epsilon} = 4.15$. From Fig. 16, it is found that the removal is 65.7%—a reduction due to turbulence of $\frac{4.3}{70}$ or 6.1% in the removal.

As another example, let it be required to find the effect of turbulence on the removal of particles whose settling velocity w is 0.22 cm per sec, settling to take place in a 200-ft settling zone, 10 ft deep in which the mean velocity is 10 ft per min and f is 0.032. The detention period, t, is $\frac{200}{10} = 20$ min or 1,200 sec. The overflow rate $w_o = \frac{10 \times 30.48}{1{,}200} = 0.254$ cm per sec. The removal in the absence of turbulence is $\frac{w}{w_o} = \frac{0.22}{0.254} = 0.866$ or 86.6%. From Eq. 53*a*, $\frac{w\,h}{2\,\epsilon} = 7.5 \frac{0.22}{10 \times \frac{30.48}{60}\sqrt{\frac{0.032}{8}}} = 5.13$. From Fig. 16 it is found that the removal is about 77.2%, the effect of turbulence being to reduce the removal about 10.8%.

Mr. Dobbins has tackled a difficult problem with ingenuity and resourcefulness. He has overcome many obstacles in mathematical analysis and experimental technique and has evolved a sound answer which must serve as a basis for future work in this field. The writer contributed the idea which initiated this program and helped with advice from time to time. It is gratifying to have been associated with the author in this small way. Even the kibitzer is pleased when the hand is won.

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AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

DISCUSSIONS

RELATION OF UNDISTURBED SAMPLING TO LABORATORY TESTING

Discussion

By Messrs. Karl Terzaghi, and D. P. Krynine

Karl Terzaghi,[8] M. Am. Soc. C. E.[8a]—An up-to-date inventory of what is known or believed concerning the influence of the disturbance due to sampling operations on the physical properties of cohesive soils has been presented by Professor Rutledge. Since the growing realization of the importance of this influence constitutes a vital phase in the development of soil mechanics, some data concerning the historical background of this phase may be of interest.

The credit for the discovery of the mechanical effects of remolding belongs to the Swedish Geotechnical Commission. The final report of the commission was published in 1922 (26).[8b] The report contains many examples of the mechanical effects of remolding and the description of a method for expressing the sensitivity of the structure of the clay by numerical values. Opinion concerning the physical causes of the observed phenomena still is divided. In 1932, Professor Casagrande proposed a hypothesis according to which the sensitivity of the structure of the natural, undisturbed clays is due to a peculiar arrangement of the clay particles, which he called the clay structure. On the other hand, the writer believed—and still believes—that it is chiefly due to slow physico-chemical processes which take place in any clay. Fortunately the issue is only of theoretical interest.

The work of the Swedish Commission initiated the development of the technique for securing undisturbed samples. The first sampling tool of the modern type was proposed by John Olsson in 1925 (27). However, since undisturbed sample borings are much more expensive than the traditional wash or auger borings, it required many years for engineers and contractors to accept the new methods of sampling. In the meantime, investigators had to

Note.—This paper by P. C. Rutledge, Assoc. M. Am. Soc. C. E., was published in November, 1942, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: December, 1942, by Messrs. Benjamin K. Hough, Jr., and F. M. Van Auken; March, 1943, by Jacob Feld, M. Am. Soc. C. E.; and April, 1943, by Messrs. Raymond F. Dawson, and Hamilton Gray.

[8] Winchester, Mass.

[8a] Received by the Secretary April 26, 1943.

[8b] Numerals in parentheses, thus: (26), refer to corresponding items in the Bibliography (see Appendix in the paper), and at the end of discussion in this issue.

be content with whatever samples they could afford. Even the so-called drive samples were considered a luxury.

Several years after the publication of the Swedish report, the writer investigated the influence of remolding on the modulus of elasticity of clays (28). In those days, consolidation tests still were made on completely remolded samples. In order to correct for the influence of remolding on the relation between pressure and void ratio, the graphical method illustrated in Fig. 13 was used (29). In Fig. 13 the symbol e_0 indicates the initial void ratio and p_0,

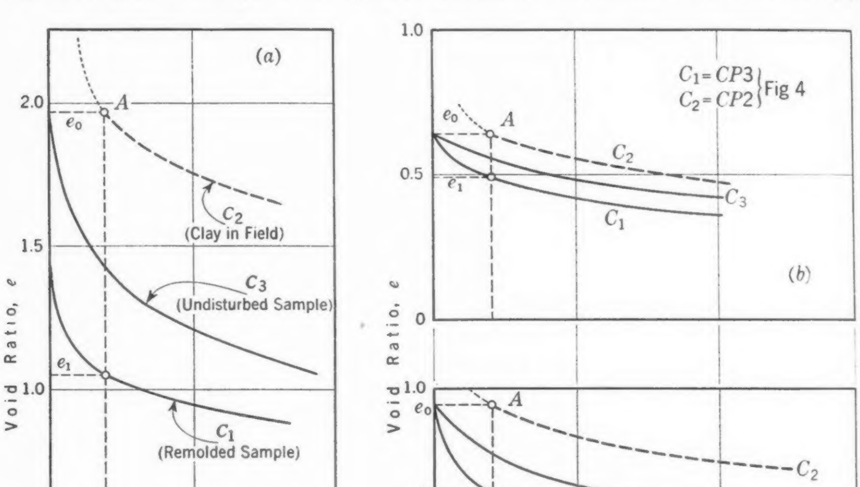

FIG. 13.—GRAPHICAL METHOD FOR ESTIMATING THE RELATION BETWEEN PRESSURE AND VOID RATIO FOR NORMALLY CONSOLIDATED CLAY IN THE FIELD (CURVES C_2) ON THE BASIS OF RESULTS OF CONSOLIDATION TESTS ON REMOLDED SAMPLES (CURVES C_1)

the vertical unit pressure which acts on the clay in the ground at the depth where the sample was obtained. The curve C_1 represents the results of a consolidation test on a remolded sample. On this curve the pressure p_0 corresponds to a void ratio e_1 which is much smaller than the void ratio e_0. The difference between e_0 and e_1 represents the effect of remolding on the void ratio at a given unit pressure p_0. It seemed obvious that the clay in its natural state, with a void ratio e_0 at a pressure p_0, should be more compressible than the remolded clay, with a smaller void ratio e_1 at the same pressure p_0. Therefore, the writer assumed that the pressure, void-ratio curve for the clay in the ground can be determined approximately by multiplying the ordinates of the C_1 curves by $\frac{e_0}{e_1}$. Thus he obtained the curves C_2.

Curves C_1 in Fig. 13 were plotted by means of the data contained in Figs. 3 and 4. The corresponding pressure, void-ratio curves for undisturbed samples are represented by curves C_3.

In 1932 Professor Casagrande published, for the first time, the results of consolidation tests on undisturbed clay samples. Curves C_3, Fig. 13, are examples of the general character of the curves thus obtained. In the diagram they occupy a position intermediate between the pressure, void-ratio curves for the remolded samples and curves C_2 which represent very roughly the pressure, void-ratio curves for the clay in the ground. The difference between curves C_2 and C_3 is a measure of the increase of the compressibility of the clay due to the disturbance of the structure by sampling operations. Settlement computations require a knowledge of the relation between p and e for the clay in the ground. In order to determine this relation on the basis of a consolidation test on undisturbed samples (curves C_3, Fig. 1), certain assumptions must be made. In the paper these assumptions are illustrated by Figs. 6, 7, and 8.

To the general reader, these diagrams may be somewhat confusing because no distinction is made between what is known and what is merely believed. Each of the diagrams contains two rebound curves. The upper one, labeled (2) in Fig. 7, is assumed to represent the rebound during sampling and storage before test. It is generally agreed that the increase of the void ratio of the clay during sampling is insignificant and that, after the sample is taken out of the hole, its water content is kept constant. Therefore, the processes that occur during sampling and storage should appear in the diagrams as a straight, horizontal line whose ordinates are equal to the initial void ratio e_0 of the clay; yet in the diagrams the line which corresponds to these processes rises as steeply as the laboratory rebound curve (curve (7), Fig. 7), which represents the swelling of the clay in contact with free water after the removal of a load.

If the upper rebound curve is eliminated and replaced by a straight line with the ordinates e_0, all the other constructions shown in the diagrams become more or less meaningless. The writer proposes that they be replaced by the simple diagram Fig. 14, which leaves no doubt about what is known and what is believed. In Fig. 2(*a*) (which shows the relation between pressure and void ratio for different samples of a normally consolidated clay) the full-line curves represent empirical curves. The dashed and dotted curves are lines obtained by inference.

While the pressure on the clay increased in the field during the process of sedimentation from its initial value, zero, to the final value p_0, the void ratio of the clay decreased as indicated by the gently sloping curve ab. This curve is an empirical, and not a hypothetical, one, as suggested by the label in Fig. 8, because it can be constructed, without any assumptions, by combining two sets of empirical data. One of them is the water-content profile of the stratum, and the second is the profile that shows the relation between depth and the overburden pressure p_0. The process of sampling and the period of storing are represented by the horizontal line bc. Curve cd_1e_1 in Fig. 14 shows the results of a consolidation test on an undisturbed sample and curve cd_2e_2 those of a test on a remolded sample. In order to obtain the relation between pres-

sure and void ratio for the clay in the ground, the straight line be is traced through point b, parallel to the straight part d_1e_1 of the pressure, void-ratio curve cd_1e_1 for the undisturbed sample. There is no theoretical justification for this method. Therefore, the procedure can be advocated only on the strength of empirical arguments. If the weight of a structure increases the

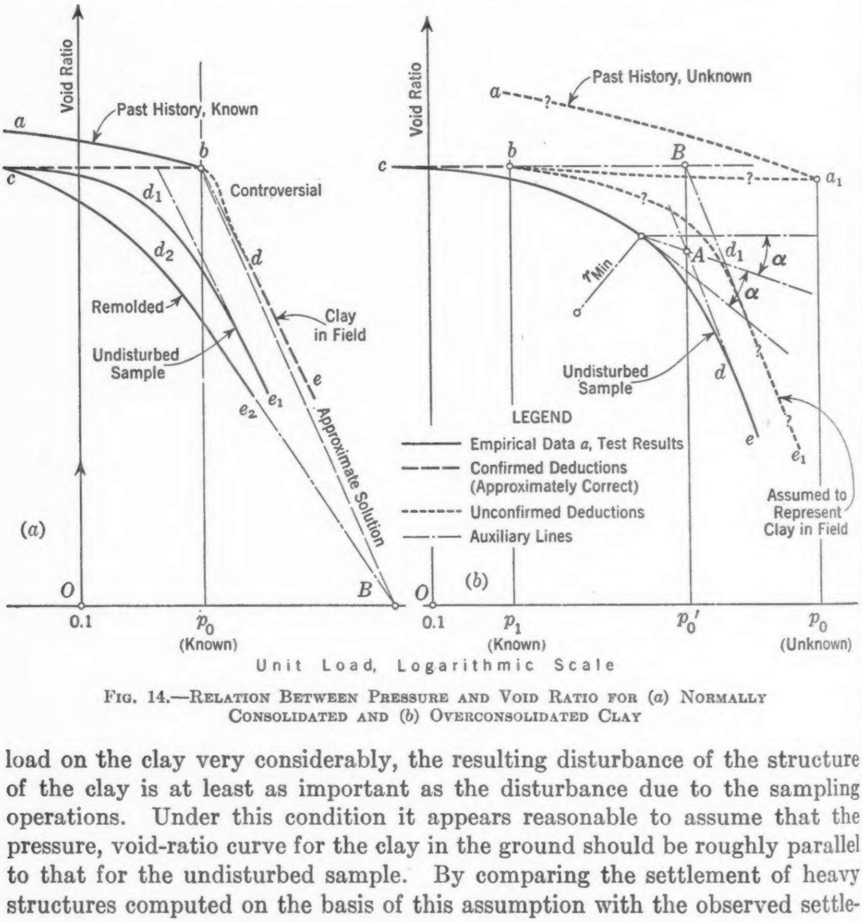

FIG. 14.—RELATION BETWEEN PRESSURE AND VOID RATIO FOR (*a*) NORMALLY CONSOLIDATED AND (*b*) OVERCONSOLIDATED CLAY

load on the clay very considerably, the resulting disturbance of the structure of the clay is at least as important as the disturbance due to the sampling operations. Under this condition it appears reasonable to assume that the pressure, void-ratio curve for the clay in the ground should be roughly parallel to that for the undisturbed sample. By comparing the settlement of heavy structures computed on the basis of this assumption with the observed settlement, it was found that the agreement is usually satisfactory.

On the other hand, if the increase of the load on the clay due to the weight of a structure is small, the analogy between the clay in the ground and the undisturbed sample does not exist, and no reliable data concerning the settlement of light structures on clay foundations are available. Therefore the upper part of the line bd in Fig. 14(*a*) has been marked as controversial.

If a job involving a foundation above a stratum of normally consolidated clay is not important enough to justify the expense of securing undisturbed samples, the consolidation tests can be made on remolded samples. Each test yields a pressure, void-ratio curve similar to curve cd_2e_2 in Fig. 14(*a*).

The continuation of the straight part d_2e_2 of this curve intersects the horizontal axis of the diagram ($e = 0$) at point B. In most instances, the slope of the line bB is somewhat greater than that of the line be, obtained from a consolidation test on an undisturbed sample; but the difference is not important enough to be of any practical consequence on a routine job. This approximate method of evaluating the relation between pressure and void ratio is identical with the method illustrated by Fig. 13. However, in Fig. 13 the pressures have been plotted on a natural, and in Fig. 14(*a*) on a logarithmic, scale.

Fig. 14(*b*) shows the relation between pressure and void ratio for an overconsolidated clay. The symbol p_0 denotes the highest pressure that acted on the clay during its geological history. It is greater than the present overburden pressure p_1. The changes of the void ratio which occurred during the gradual rise of the pressure from zero to p_0 and the subsequent decrease from p_0 to p_1 are not known. Therefore they are indicated by dotted lines, aa_1 and a_1b. The value of p_0 can only be guessed at on the basis of geological data. The changes that occurred during sampling and storing are shown by the broken horizontal line bc. The results of a consolidation test on an undisturbed sample are shown by curve cde.

By means of the graphical construction indicated in Fig. 14(*b*), a point A is obtained which is located on the straight-line continuation of curve de (30). The pressure $(p_0)'$ which corresponds to the abscissa of this point is assumed to be roughly equal to the preconsolidation pressure p_0. The controversial issues associated with this assumption have been discussed by the writer elsewhere (31). Since the author has made a considerable number of consolidation tests on shaft and tunnel samples from Chicago, Ill., and other localities, he could assist materially in clarifying these issues by presenting, in his closure, a list of the preconsolidation loads that were obtained for these samples, together with the data which determine the position of the samples with reference to the water table and to the ground surface. The importance of the difference between $(p_0)'$ and the real preconsolidation pressure p_0 cannot yet be estimated; but it is fairly certain that the ratio $\frac{(p_0)'}{p_0}$ depends to a large extent on the method of sampling. The greater the disturbance of the clay due to sampling, the smaller will be the ratio $\frac{(p_0)'}{p_0}$, all other factors being equal. The ratio $\frac{(p_0)'}{p_0}$ may also depend on the depth at which the samples were secured.

The next step consists in tracing a vertical line through point A. This line intersects the right-hand continuation of bc at point B. Finally Be_1 is traced parallel to de. The pressure, void-ratio curve for the clay in the ground is assumed to be tangent to Be_1, and the upper part bd_1 of this curve is assumed to have the same trend and curvature, roughly, as the upper part of the laboratory consolidation curve.

The most essential requirement for the application of the procedure illustrated by Fig. 14(*b*) is to secure first-class undisturbed samples, because a slight disturbance of the structure of the clay effaces almost completely the traces of the loading history of the clay. Even if the samples have been se-

cured with utmost care in test shafts, however, the procedure still leaves ample margin for error. Therefore, it is not surprising that the agreement between the results of the settlement computations for structures on overconsolidated clays and the true settlements are rather unsatisfactory in most instances, whereas the settlement computations for heavy structures on normally consolidated clays consistently are found to be reliable.

The two diagrams in Fig. 14 are so simple that no special terminology is required; yet they show everything that is really essential.

Triaxial compression tests are an inexhaustible source of information concerning the behavior of soils subject to stress in space under laboratory conditions; but the practical benefits of these investigations cannot be reaped until the difference between field and laboratory behavior has been explored by means of field observations on full-sized structures. At present the evaluation of this difference still is based chiefly on opinion.

In the last part of his paper, the author mentions semi-empirical methods for dealing with foundation problems. The essence of these methods consists in adapting the fundamental assumptions of the theories to the results of observations in the laboratory and in the field. As experience increases, the methods are modified and generalized in accordance with findings. The Swedish circular-arc method is a good example. After the method was devised, it was found to be not applicable to the failure of fills above thin clay strata. Therefore it was necessary to supplement it by a method based on assuming the possibility of a failure along a composite surface of sliding. For similar reasons, Coulomb's theory of earth pressure had to be expanded into the general wedge theory (32). Because of its inherent adaptibility, a semi-empirical method does not deserve its name unless it agrees, at any time, with the experience which has been accumulated up to that time. The theories of bearing capacity which the author quotes as examples were published many years ago. Therefore they should be discounted until they are again brought up to date.

On account of the impressive diversity of its contents, the paper contains a vast amount of food for thought. Therefore, it will not fail to exert a stimulating influence on many readers.

D. P. KRYNINE,[9] M. AM. SOC. C. E.[9a]—The material testing laboratory is closer to the activities of an engineer when he deals with the foundations of a structure than when he works on the structure itself. This is because the medium surrounding the foundation, and acting together with it, cannot be purchased or ordered to satisfy certain specifications. Some evidence must be furnished by the soil laboratory in each particular case. The writer wishes to emphasize the word "some" in the preceding sentence. Every engineer understands very clearly that a soil laboratory does not test the earth material itself, but merely furnishes information on the properties of the samples tested, under the limitation, of course, that proper tests have been performed properly. In this connection an authoritative and frank explanation of the value of soil

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[9a] Received by the Secretary May 25, 1943.

samples and of the tests themselves, as issued by Professor Rutledge, should be welcomed by the profession.

Consolidation Test.—To determine how much a deep clay layer will decrease in thickness (settle) due to the pressure from a proposed structure, a model of this layer (an "undisturbed" thin soil sample) is tested. It is subjected to the same conditions as the natural clay layer so far as the initial pressure p_1 (before construction) and the final pressure p_2 (after construction) are concerned. The main object of the test is to find the relationship between the "void ratio" of the clay (ratio of the volume of voids to the ratio of solids) and the applied unit pressure. On semilogarithms paper, this relationship is approximately expressed by a straight line termed "virgin compression curve" (Fig. 1(*b*)). It is assumed that the change in void ratio, $e_1 - e_2$, due to an increment in pressure, $p_2 - p_1$, is the same for the model and the prototype. The final settlement (or the decrease in thickness) of the layer may be computed easily from the value $e_1 - e_2$; and the steeper the "virgin compression curve," the larger the expected settlement will be.

The "virgin compression curve" is supposed to be tangent to the laboratory compression curve (Fig. 1(*b*)). The latter is obtained in the laboratory by applying rather large increments of load at a rather quick rate and the test cannot be done otherwise. According to Professor Terzaghi (10) in doing so, the laboratory worker breaks "rigid clay bonds" and transforms the clay of the sample into a soft ("lubricated") mass. In nature load increments are applied slowly—grain by grain, particle by particle. Hence the field "virgin compression curve," before construction, is flat (curve (2), Fig. 8) whereas the laboratory curve (curve (7), Fig. 8) is steep. What happens in the natural clay layer after the construction is not known; but it may be assumed that what may be termed the "true compression line" is somewhere between the continuation of curves (2) and (7) in Fig. 8. Professor Rutledge shows it under the form of curve (4), Fig. 8, close to the laboratory compression curve. The writer disagrees with this visualization for the simple reason that, although the load increments are equal in the model and in the prototype, the rate of application is about fifty times slower in the latter case than in the former. In fact, the same final unit load is reached perhaps in one week in the laboratory and roughly in one year on the construction job. Besides, no one can be sure whether the mechanics of this breaking of bonds, if any, is the same in a sample $\frac{1}{2}$ in. or 1 in. thick and in a clay layer, say 40 ft thick. Hence the writer believes that curve (4), Fig. 8, should be flatter.

Thus the settlement analysis using laboratory data apparently furnishes exaggerated values of settlement, the order of magnitude of this exaggeration being uncertain. Of course, the settlements thus computed are always on the safe side; but this safety is of such a nature as to permit an experienced engineer to believe, and with some reason, that a simple guess of the value of the expected settlement may be not less correct than the result obtained in most elaborate tests.

However, the writer is hopeful about the future of the consolidation test. Professor Terzaghi is to be commended highly for publishing the theory cited

(10). The theory may prove incorrect, in which case everything will remain as it was before the publication of Professor Terzaghi's paper. If the theory is correct (and probably it is correct), steps should be taken to correlate the laboratory tests with that theory, and this undoubtedly will be done.

Shear Tests.—Professor Rutledge discusses the direct shear test, the triaxial test, and the unconfined compression test, and apparently prefers the latter. This is indeed a promising simple and nonexpensive test performed on prismatic or cylindrical specimens cut out from large cubes taken by hand or from undisturbed samples extracted from certain depths using samplers. Any disturbance of the sample furnishes results on the safe side; but evaporation (drying out of the specimen during the test) unduly increases the shearing strength of the sample and should be avoided.

During the Annual Meeting of the Society in January, 1943, M. Juul Hvorslev, Assoc. M. Am. Soc. C. E., demonstrated his apparatus for making the unconfined test on tiny prisms or cylinders cut from undisturbed samples. It would be interesting to know Professor Rutledge's opinion of this method.

A worth-while addition to the paper by Professor Rutledge is a discussion by A. E. Cummings, M. Am. Soc. C. E. (33), in which the comparison between the unconfined compression test and the ring test by W. S. Housel, M. Am. Soc. C. E., is thoroughly discussed. The ring method itself (34) consists in pulling out a slice from a cylindrical undisturbed sample. In other words, this is double shear as in the case of a rivet which works in that manner.

Coulomb Formula.—This is another important soil mechanics item which, together with the consolidation test, passes now (1943) through a crisis. It is rightly stated by Professor Rutledge (35) that: "The results of shear tests must eventually be reduced to the Coulomb equation or a simple modification of it." The Coulomb formula states that the shearing resistance (shearing strength) of a soil, s, is the sum of its unit cohesion, c, visualized for a given soil as a constant, and the product of the normal pressure, σ, acting at the given point, by a certain coefficient depending on the properties of the given soil. This coefficient is visualized as a tangent of an angle ($\tan \phi$), the angle ϕ being termed "angle of internal friction," "effective angle of internal friction," or "angle of shearing resistance." It follows from the Coulomb formula that the shearing resistance should increase with the depth of the deposit, since the normal pressure, σ, depends on the weight of the overburden; but this is not always the case. Some engineers believe that the angle of internal friction for plastic clays, ϕ, is equal to zero, which means that the shearing strength is constant throughout a mass of plastic clay. Some others, including Professor Rutledge (see paragraph following Eq. 2), believe that "the increases in test strengths * * * which have been attributed to an angle of internal friction in undisturbed clay, may, in fact, be due entirely to decreases in void ratio." In other words it is believed that the smaller the volume of the voids, the larger the shearing strength of the clay will be.

The writer believes that the shearing strength of a clay mass may be practically constant throughout, due to the peculiar structure of some clays which

prevents consolidation. Another possibility is the reinforcement of a clay deposit by calcium carbonate (or other chemical) due to the action of fluctuating ground water as in some places in Arizona. Furthermore, the writer wishes to warn against attributing the increase in shearing strength during consolidation solely to the decrease of the volume of voids. Possibly, the increase in shear strength in such cases is due mostly to the interaction of mutually approaching surfaces and hence may be influenced by adding chemicals which change the surface conditions. In this connection acquaintance with the work by Prof. H. F. Winterkorn, Assoc. M. Am. Soc. C. E., may be helpful (36).

Runways and Highways.—The paper by Professor Rutledge considers primarily the tests used in the study of foundations of heavy structures such as bridges and tall buildings. The field of tests which are necessary for runways and highways is excluded from consideration. It is true that one of the most widely used tests in that field—the so-called "California bearing ratio test" does not require undisturbed samples. In this test moistened soil material is consolidated in a mold and tested for penetration; but capillary tests and tests of permeability may be made both on disturbed and undisturbed samples, and comparison of both may be of importance. In this connection some acquaintance with the theory of "capillary potential" used by soil scientists may be of interest (37).

Terminology.—In the opinion of the writer the term "idealized" is very correctly used in this paper. An "idealized" material or object is assumed to possess properties or qualities which in reality it possesses only in part or to some extent; or, possibly, it lacks entirely. The virgin compression curve in Fig. 2 may or may not be a straight line (as is the case of some clays); but in this paper it is "idealized" and visualized in the form of a straight line. Eq. 1 obviously refers to the "idealized" virgin compression line and not to the actual curve. If an able draftsman happens to trace a straight line, as in Fig. 2 or elsewhere, in an irreproachable manner, it will be an "ideal" straight line.

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AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

DISCUSSIONS

STATISTICAL ANALYSIS IN HYDROLOGY

Discussion

BY E. J. GUMBEL, ESQ.

E. J. GUMBEL,[40] ESQ.[40a]—Two important questions have been raised by Mr. Beard:

(*a*) What are the adequate statistical theories available for distributions observed in hydrology; and, especially, which theoretical distribution must be chosen for the largest observed values; and

(*b*) How should the observed frequencies be corrected to account for the small number of observations.

The writer wishes to show first that the solutions proposed by Mr. Beard are too general and, therefore, inadequate for specific problems. In the course of this proof he proposes a general method for obtaining the corrections. The actual corrections to be used depend upon the theoretical distributions chosen for representing the observations.

Theoretical Distributions.—There is no general solution to the quest for theoretical distributions to be applied to hydrological observations. In principle, such data are statistical observations. The difference from other statistical observations is a function of specific conditions such that the usual statistical methods cannot always be applied.

Certain hydrological distributions are subject to the normal probability law. In some cases, this will hold only for transformed variables. Mr. Beard has given an example of a logarithmically transformed normal distribution. It is a question of definition whether this still can be called a "normal distribution." Other distributions may be exponential, or of the Pearson type. Mr. Beard's contention that only the normal law is a true probability law is not valid since it denies the entire progress of statistics during the past three decades.

NOTE.—This paper by L. R. Beard, Jun. Am. Soc. C. E., was published in September, 1942, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: October, 1942, by Messrs. L. Standish Hall, and H. Alden Foster; November, 1942, by Messrs. Paul V. Hodges, and Joe W. Johnson; December, 1942, by R. H. Blythe, Jr., Esq.; and February, 1943, by Messrs. R. W. Davenport, Edward J. Bednarski, and Ralph W. Powell.

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[40a] Received by the Secretary April 9, 1943.

Mr. Beard believes that the distribution of the flood discharges is normal. In reality, numerous observations on many streams have shown that the distribution of the flood discharges is skew. The distribution rises quickly to a maximum—the most probable annual flood discharge, the mode—and reduces slowly. The mean annual flood discharge is found to be larger than the mode. The cumulative frequency converges more slowly toward unity than is the case with the normal curve.

To obtain the theoretical distribution of the flood discharges, one must realize how this distribution is observed. The largest of the 365 observations of the daily discharges herein termed "flood" is selected and repeated for n years. In general, the largest among N observations is selected, repeated n times, and an observed distribution of the largest value is thus obtained. The corresponding theoretical distribution is obtained as follows: Let $W(x)$ be the probability of a value equal to, or less than, x; let $w(x) = W'(x)$ be the density of probability (herein termed "distribution"). Then the probability that N observations will be equal to, or smaller than, x is $[W(x)]^N$. This is the probability that x will be the largest among N observations. Therefore, for an initial distribution $w(x)$ the distribution $w_N(x)$ of the largest value is

$$w_N(x) = N[W(x)]^{N-1}\, w(x) \quad \text{.................. (12)}$$

The largest value is a statistical variable with a distribution of its own, which, in general, is different from the initial distribution, from which the largest values were taken. If the initial distribution is known, it is possible to calculate the distribution of the largest value, as a function of N. This has been done by L. H. C. Tippett[41] for the normal distribution and for small values of N. To obtain the limiting distribution of the largest value that is valid for large numbers of observations N, the variable x in the initial distribution $w(x)$ is assumed to be unlimited. This holds for the normal law, and for most of the aforementioned distributions.

At first, this idea of unlimited variables sounds paradoxical. For the practical purpose of reproducing the observations, the distinction between limited and unlimited variables does not play an important rôle as the theoretical distributions converge rapidly toward zero; but the distinction is important in dealing with the largest value. If the initial variable is unlimited, the largest value also is unlimited. From the very moment that the idea of unlimited variables is accepted and the assumption is made that the distribution of the daily discharges belongs to this type, there is no such thing as a maximum possible flood.

Different authors[42] have shown, by different methods, that the probability $[W(x)]^N$ converges for initial distributions of an unlimited variable subject to few restrictions toward

$$W^*(x) = \text{Exp}\,[-\,\text{Exp}\,(-\,y)] \quad \text{.................. (13)}$$

[41] "On the Extreme Individuals and the Range of Samples Taken from a Normal Population," by L. H. C. Tippett, *Biometrika*, December, 1925, p. 364.

[42] See Bibliography in "The Return Periods of Flood Flows," by E. J. Gumbel, *The Annals of Mathematical Statistics*, Vol. XII, No. 2, June, 1941, p. 163.

in which the reduced dimensionless variable y is

$$y = \alpha\,(x - u) \qquad (14)$$

The two parameters u and $\frac{1}{\alpha}$ depend upon the initial distribution $w(x)$ and upon the number N. Both have the dimension x. They will be interpreted subsequently as a mean value and as a measure of dispersion, respectively.

For the normal curve, the probability of a value less than three times the standard deviation is 0.99865, whereas $W^*(x)$ reaches this value only for five times the standard deviation. The distribution $w^*(x)$ of the largest value, obtained from Eq. 13 by differentiation, is

$$w^*(x) = \alpha\,\mathrm{Exp}\,[-y - \mathrm{Exp}\,(-y)] \qquad (15)$$

This is a skew distribution of an unlimited variable. All moments converge. The distribution has a maximum for $y = 0$, that is, $x = u$. This is the mode. The mean value of the reduced variable is $\bar{y} = 0.57722$.

If the two parameters are estimated from the observed distribution of the largest value, Eq. 15 may be used even when the initial distribution $w(x)$ is unknown. The parameter u is the most probable largest value for N observations. If Eq. 15 is applied to the flood flow, u is the most probable annual flood. It may be estimated through the relation

$$u = \bar{u} - 0.58721\,\theta \qquad (16a)$$

in which $\bar{u}$ stands for the mean annual flood. The value θ, the "mean deviation," is the mean of the absolute deviations calculated from the mean annual flood.

The other parameter $\frac{1}{\alpha}$ has been shown[43] to be

$$\frac{1}{\alpha} = 1.01731\,\theta \qquad (16b)$$

Another interpretation of $\frac{1}{\alpha}$ is obtained by introducing the return period. For observations recorded in time, the theoretical return period $T(x)$ of a value equal to, or larger than, x is defined by

$$T(x) = \frac{1}{1 - W(x)} \qquad (17)$$

In treating flood discharges, the return period is counted in years, and $W(x)$ is replaced by $W^*(x)$. Combining Eqs. 13 and 14 with Eq. 17, the return period of a flood, equal to, or larger than, x, is found to converge for large values of x toward

$$\log_e T(x) = \alpha\,(x - u) \qquad (18)$$

Eq. 18 holds with sufficient accuracy for observations of twenty years and more. Under this condition, the flood discharge x occurring in Eq. 18 is the most

[43] "Statistical Control—Curves for Flood Discharges," by E. J. Gumbel, *Transactions*, Am. Geophysical Union, 1942, p. 489.

probable flood discharge corresponding to the return period $T(x)$. The logarithm of the return period is a linear function of the most probable flood discharge. The parameter is the slope of this straight line. Furthermore, from Eq. 18, if the return period is multiplied by e, the most probable flood increases by $\frac{1}{\alpha}$.

The theory of largest values accommodates well the observations taken from the Columbia, Tennessee, Mississippi, Cumberland, Connecticut, Rhine, and Rhône rivers.[42,43,44] Thus answering the first question raised by the paper, this theory should be chosen for the observed distribution of the flood discharges.

Corrections of the Observed Frequencies.—The second question is: Which corrected frequency and return period shall be attributed to the mth value for an arbitrary number of observations n, and especially to the mth flood discharge?

Let $x_1, x_2, \cdots x_m \cdots x_{n-1}, x_n$ be observations arranged in increasing order of magnitude. The index m is the serial number. To the mth observation x_m, the observed frequency $\frac{m}{n}$ is attributed; but it is equally legitimate to arrange the observations in a decreasing order of magnitude. The mth observation increasing is the $(n - m + 1)$th observation decreasing. The observed frequency decreasing is $\frac{n - m + 1}{n}$, which corresponds to an observed frequency $\frac{m - 1}{n}$ increasing.

Just as there are two ways of counting the observations, there are two ways of plotting the cumulative frequency curve (the duration curve), which represents the absolute or relative number of observations smaller than, or larger than, a certain value. In the first case, the curve starts with 0 and ends with 100%—the total number of observations. (Fig. 2 ends with 44%, whereas Mr. Beard states correctly that it should end with 100%.) In the second case, the frequency curve starts with 100%, and ends with 0.

In the same sense as for the frequencies, there are two ways of plotting the observed return periods, namely, the so-called exceedance intervals

$$T'(x_m) = \frac{n}{n - m} \qquad (19)$$

defined for $1 \leqq m \leqq n - 1$, and the recurrence intervals

$$T''(x_m) = \frac{n}{n - m + 1} \qquad (20)$$

which may be plotted on probability paper for $2 \leqq m \leqq n$.

For small serial numbers, the differences between the two ways of plotting are small as the return periods then are approximately

$$T = 1 + W \qquad (21)$$

[44] "Probability-Interpretation of the Observed Return-Periods of Floods," by E. J. Gumbel, *Transactions*, Am. Geophysical Union, 1941, p. 836.

in which W is nearly zero and T nearly unity; but, for large serial numbers, especially for the extreme values, the differences between the two ways of plotting are large. For the penultimate value the two observed return periods are n and $\frac{n}{2}$, respectively.

To solve this dilemma Mr. Beard assumes that the observed mth value is the median of all possible mth values. This is certainly legitimate; but he constructs an analogy of any observed distribution with a binominal distribution. In reality, the probability of an observed mth value taken from an arbitrary distribution cannot be identified with the probability that an event happens $m - 1$ times in an alternative. He advocates the same correction to the mth value, decreasing and increasing, independent of whether the distribution is symmetrical or not. Mr. Beard calculates the corrections for ten observations. It would be difficult to use his method for an arbitrary number of observations, as this requires the solution of $\frac{n}{2}$ equations of a complicated type. His proposal has no logical basis; and, moreover, is impracticable. Mr. Beard seems to have recognized the second drawback. Therefore, he proposes, as an alternative, the Foster-Hazen[45] correction which consists of a compromise between m and $m - 1$, and which attributes a frequency $m - \frac{1}{2}$ to the mth observation. The corrected intervals

$$T(x_m) = \frac{n}{n - m + \frac{1}{2}} \qquad \text{(22a)}$$

may be used for $1 \leqq m \leqq n - 1$. The return period of the $(n - m + 1)$th observation (which is the mth observation, decreasing) is then

$$T(x_{n-m+1}) = \frac{2\,n}{2\,m - 1} \qquad \text{(22b)}$$

It seems doubtful whether Eq. 22*b* can be used for the last value. In this case, one attributes the return period

$$T(x_n) = 2\,n \qquad \text{(22c)}$$

to the largest among n observations. In reality, only n observations are available, and no statistical device can increase the number of observations beyond n. Besides, it does not seem safe to attribute a return period $2\,n$ to an event which already has happened within n years.

According to Eq. 22*b* the return period of the mth value (decreasing) for n observations is equal to the return period of the $(m + 1)$th value for $n\,\frac{2\,m + 1}{2\,m - 1}$ observations. This relation is by no means satisfactory; common sense leads to another, simpler idea. The return period of the last value for n observations should be equal to the return period of the penultimate value for $2\,n$ observations.

Messrs. Hazen and Beard infer that a general correction for any theoretical distribution is valid. The writer's solution is based on the following considerations:

45 "Flood Flows," by Allen Hazen, John Wiley & Sons, Inc., New York, N. Y., 1930.

(*a*) The correction must decrease with the number of observations n. This relation should be stated in such a way that the correction can easily be calculated as a function of m and n.

(*b*) For symmetrical distributions the corrections for two observations that are symmetrical about the mean must have symmetrical properties. As this will not hold for an asymmetrical distribution, the corrections must depend upon the theoretical distribution.

The mth value among n observations taken from an initial distribution $w(x)$ is a statistical variable which has a distribution $v_n(m, x)$, depending upon m and n and $w(x)$. The mth observation is assumed as a mean value $\mathbf{x}_m$ of the distribution $v_n(m, x)$. Therefore, one attributes to the mth observation the probability $W(\mathbf{x}_m)$ of this mean considered as one of the values of the initial variable.

For n observations, the mth value increasing is such that there are $m - 1$ values before the mth value and $n - m$ values after the mth value. These probabilities are, respectively, $[W(x)]^{m-1}$ and $[1 - W(x)]^{n-m}$. The mth value has the density of probability $w(x)$. Finally, the number of combinations must be taken into account. Therefore, the distribution $v_n(m, x)$ is

$$v_n(m, x) = \binom{n}{m} m\, [W(x)]^{m-1}\, [1 - W(x)]^{n-m}\, w(x) \quad \ldots\ldots\ldots (23)$$

The most probable mth value $\mathbf{x}_m$ obtained by logarithmic differentiation is the solution of

$$m - 1 - (n - 1)\, W(\mathbf{x}_m) = - \frac{w'(\mathbf{x}_m)}{w^2(\mathbf{x}_m)}\, W(\mathbf{x}_m)[1 - W(\mathbf{x}_m)] \quad \ldots\ldots (24)$$

The probability $W(\mathbf{x}_m)$ of the most probable mth value is a function of m, n, and the initial distribution $w(x)$. It is reasonable to assume that the observed mth value is the most probable one. Then one must attribute to the observed mth value x_m the corrected frequency, obtained from Eq. 24:

$$\mathbf{W} = W(\mathbf{x}_m) \quad \ldots\ldots\ldots\ldots\ldots\ldots (25)$$

which is the probability of the most probable mth value. Eq. 24 may be written in the form

$$m - n\, \mathbf{W} = \Delta \quad \ldots\ldots\ldots\ldots\ldots\ldots (26)$$

in which the right side

$$\Delta = 1 - \mathbf{W} - \frac{w'(\mathbf{x}_m)}{w^2(\mathbf{x}_m)}\, \mathbf{W}\, (1 - \mathbf{W}) \quad \ldots\ldots\ldots\ldots (27)$$

herein termed the "correction," depends upon the initial distribution. The corrected frequency $\mathbf{W}$ and the corrected return period $\mathbf{T} = T(\mathbf{x}_m)$ of the mth observation are

$$\mathbf{W} = \frac{m - \Delta}{n}\ ; \text{ and } \mathbf{T} = \frac{n}{n - m + \Delta} \quad \ldots\ldots\ldots\ldots (28)$$

Eqs. 28 shows that the first postulate is fulfilled.

The numerical values of the correction Δ may be calculated from Eq. 27 for selected values of the corrected frequency **W**. The serial number m as a function of **W** obtained through Eqs. 28 will not be an integer. The corrected frequencies **W** corresponding to integer numbers m are obtained by linear interpolations. If n is large, this tedious work may be avoided. The correction Δ as a function of $\frac{m}{n}$ is slightly different from Δ considered as a function of the frequency **W**. Neglecting this difference, the calculated values of Δ are attributed to $\frac{m}{n}$ instead of **W**; and, for given values of m, the corrected frequencies **W** are obtained from Eqs. 28.

By this procedure, Eq. 24 gives the corrected frequency for any number of observations, any serial number, and any initial distribution. This settles the author's second question.

To obtain the correction for symmetrical distributions, a reduced variable z is introduced in Eqs. 28—

$$z = \frac{x - \bar{x}}{\sigma} \quad \text{(29)}$$

in which $\bar{x}$ stands for the arithmetic mean and σ for the standard deviation. The probabilities $W(-z)$ and $W(z)$ are then related by

$$W(-z) + W(z) = 1 \quad \text{(30a)}$$

The same symmetry holds for the correction $\Delta(-z)$ and $\Delta(z)$. From Eq. 27:

$$\Delta(-z) + \Delta(z) = 1 \quad \text{(30b)}$$

For symmetrical distributions, the corrections show the same symmetry as the probabilities. This was the second postulate.

For an exponential distribution

$$w(x) = \alpha \operatorname{Exp}(\alpha x) \quad \text{(31)}$$

the corrected frequency of the mth observation is

$$\mathbf{W} = \frac{m - 1}{n} \quad \text{(32)}$$

and the corrected return periods are the recurrence intervals, Eq. 20. For the uniform distribution, in which $w'(x) = 0$:

$$\mathbf{W} = \frac{m - 1}{n - 1} \quad \text{(33)}$$

The differences $\frac{m}{n} - \mathbf{W}$ of the observed frequency $\frac{m}{n}$ and the corrected frequency **W** are plotted in Fig. 9 for different distributions and for ten observations. According to Hazen, the difference is equal to $\frac{1}{20}$, independent of m. The irregularities in Mr. Beard's curve are probably due to the fact that he did not calculate a sufficient number of decimals. The two straight lines for

the uniform and exponential distributions are obtained from Eqs. 32 and 33. Fig. 9 shows that, for small numbers of observations, the correction depends strongly upon the distribution. The construction of a general correction does not make sense for a small number of observations.

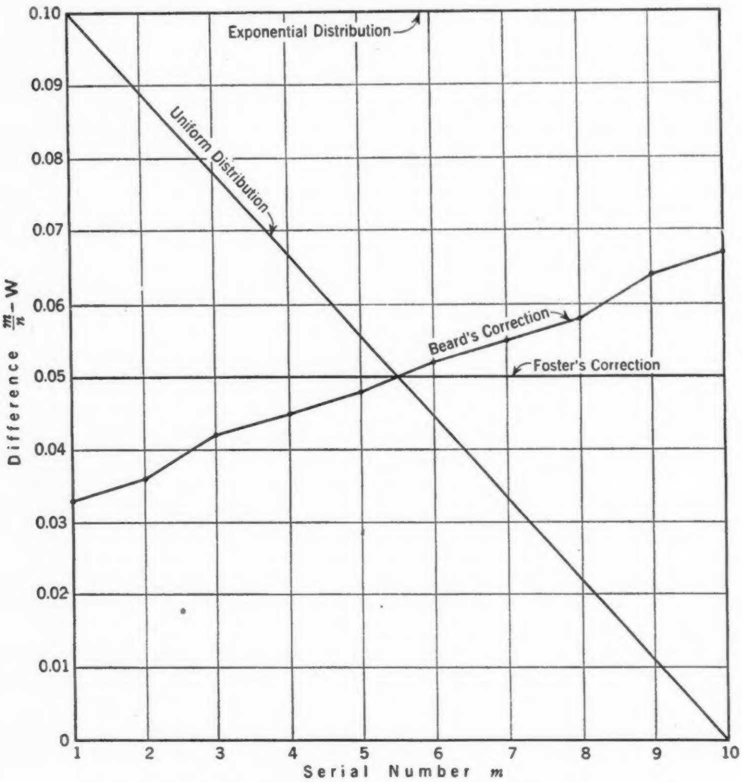

FIG. 9.—DIFFERENCES OF THE OBSERVED AND CORRECTED FREQUENCIES FOR TEN OBSERVATIONS

Application to the Flood Flow.—The remaining step is to determine the best plotting points for the flood discharges, and to decide which compromise (if any) should be made between the recurrence and the exceedance intervals. For this purpose, the theory of the largest value, Eq. 13, is used. The calculation of the corrected frequencies and return period for an arbitrary mth flood cannot be shown in the limited space available. However, for large flood discharges, the method is simple. For large values of the reduced variable y, which correspond to large flood discharges, the probability (Eq. 13) converges toward

$$W^*(x) = 1 - \text{Exp}\,(-y) \quad \ldots\ldots\ldots\ldots\ldots\ldots (34)$$

which is the probability of an exponential variable. According to Eq. 32, the corrected return periods for the largest flood discharges are the recurrence

intervals (Eq. 20). The corrected return period of the largest flood observed within n years is n; the corrected return period of the penultimate flood is $\frac{n}{2}$; and, generally, the corrected return period of the mth flood (m decreasing) is $\frac{n}{m}$. This relation does not hold for small and intermediate floods; but the engineer is mainly concerned with the largest floods. Then the simplest procedure is to make no compromise at all between exceedance and recurrence periods and to plot only the recurrence intervals. According to this method, the return period of the most probable largest flood for n years is also the return period of the most probable penultimate flood for $2\,n$ years and of the most probable mth flood (m in the descending order) for $m\,n$ years. This result is in accord with common sense, and is preferable to the complicated relation obtained from the Foster method.

The following example demonstrates that the theory of the largest value holds for the flood discharges and that the best plotting points for the largest discharges are the recurrence intervals. For the first purpose, it is sufficient generally to plot the recurrence intervals. For plotting, the probability paper described by R. W. Powell[37] is used. The flood discharges x_m are the ordinates. The abscissa is the reduced variable y; but, in addition to y, the frequencies W^*, given by Eq. 13, are written on the lower scale and the return periods T on the upper scale. The discharges corresponding to $y = 0$ and $y = 0.577$ are the most probable and the mean annual floods, respectively; but these theoretical values need not occur among the observed discharges. The possible positions of these discharges are shown by broken lines in Fig. 10.

If the theory of the largest values holds, Eq. 14 should be a straight line for the observed flood discharges x expressed as a function of y. The two constants occurring in Eq. 14 are the flood discharge u corresponding to $y = 0$, and the slope $\left(\frac{1}{\alpha}\right)$ of the straight line. The two constants may be calculated by Eqs. 16 or obtained from the graph itself.

These methods may be applied to the flood discharges of the Mississippi River observed in Vicksburg in the fifty years, 1890–1939: The observations x_m, arranged in increasing order of magnitude, are given in a previous publication.[42] The observations plotted in Fig. 10 are scattered around a straight line. This confirms the theory.

To see the influence of Foster's correction one may plot the same observations and add $\frac{1}{100}$ to each frequency. The differences between the recurrence intervals and Mr. Foster's values are negligible for small and intermediate floods. The differences become serious for the largest floods. If straight lines are drawn through the two curves so that they may be used to forecast future floods, the extrapolation from the Foster curve leads to expected floods, which are smaller than the most probable floods. Therefore, the Foster method advocated by Mr. Beard does not seem to be safe.

[37] "A Simple Method of Estimating Flood Frequency," by Ralph W. Powell, *Civil Engineering*, February, 1943, p. 105.

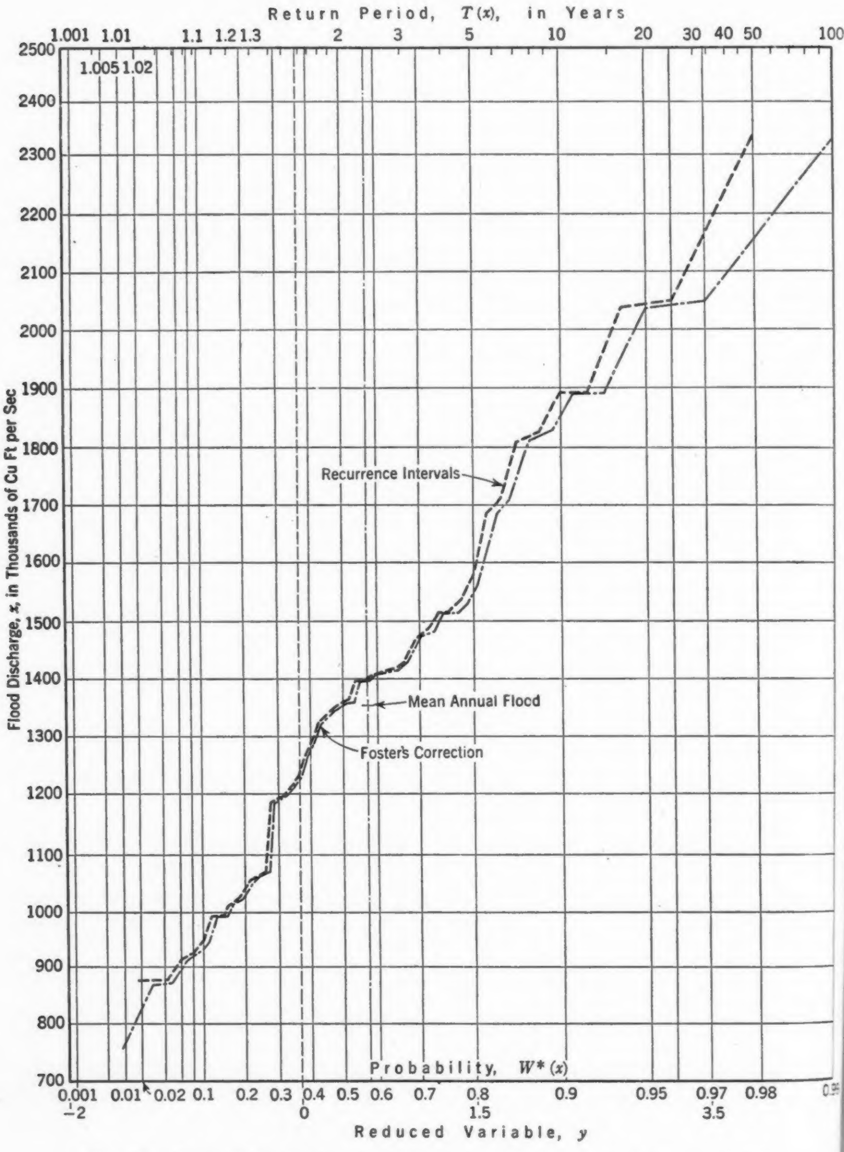

FIG. 10.—FLOOD DISCHARGES, MISSISSIPPI RIVER AT VICKSBURG, MISS., 1890–1939

To summarize: The correction of the observed frequencies is important for all observations if their number is small and, only for the largest observations, if the number of observations is large. A general correction, which holds for all distributions and for any number of observations, does not exist. To obtain the corrected frequencies, the theoretical distribution corresponding to the observations must be known. Assuming that the corrected frequency of the mth observation is the probability of the most probable mth value, the corrected frequencies and return periods for any distribution and for any number of observations are obtained from Eqs. 26 and 27. The corrections vary for different distributions.

It is safe to assume that the theory of the largest value (and not the normal distribution) holds for the flood discharges. The return periods of the most probable largest flood discharges are the recurrence intervals.

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DISCUSSIONS

PRIMARY RÔLE OF METEOROLOGY IN FLOOD FLOW ESTIMATING

Discussion

BY MESSRS. ROBERT E. KENNEDY, AND L. K. SHERMAN

ROBERT E. KENNEDY,[8] M. AM. SOC. C. E.[8a]—This paper should be read in connection with a similar paper (20)[8b] on the same subject—a bit more technical perhaps—by A. K. Showalter and S. B. Solot, who were mentioned by Mr. Bernard under "Acknowledgments." The writer is interested in the meteorological conditions described by these men as limits beyond which nature cannot go but which nature reaches quite frequently. If that is so, students of this subject are approaching the ultimate sources.

The background of approach to the problem in both these papers is to divide it into three parts: First, there is the maximum amount of moisture available for precipitation that can be brought to the region under consideration; second, there is the maximum wind velocity in the region that will feed the moisture into the storm vortex; and, finally, there is the maximum time limit of duration of the disturbance.

The first step is comparatively simple. In 1939 Mr. Solot outlined a method (32) of ascertaining the water in a column 1 sq in. in area and some miles high. From this point it is a logical step to ascertain the precipitable water in a column of saturated air and to tie it to the dew point at the surface. This was done by Messrs. Showalter and Solot (20*a*).

The moisture is brought in by some maritime air mass and is spread out like a blanket over an area extending from its source region over the ocean. Studies of air mass movements account for the disturbances that develop in this blanket. The trigger action that sets off the storm is described in both papers; but, whatever starts it, the action itself results in a maelstrom of updraft turbulence fed by the energy of latent heat released when the moisture

NOTE.—This paper by Merrill Bernard, M. Am. Soc. C. E., was published in January, 1943, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: March, 1943, by Edgar Dow Gilman, M. Am. Soc. C. E.; and April, 1943, by Messrs. Clarence S. Jarvis, Ivan E. Houk, W. G. Hoyt, and L. R. Beard.

[8] Asst. Engr., U. S. Bureau of Reclamation, Denver, Colo.

[8a] Received by the Secretary April 26, 1943.

[8b] Numerals in parentheses, thus: (20), refer to corresponding items in the Bibliography (see Appendix 1 in the paper), and at the end of discussion in this issue.

is precipitated. The updraft provides temporary storage for large quantities of the neighboring blanket of moisture fed in at high wind velocity.

Both papers represent this action by the same figure. This is Fig. 17 in Mr. Bernard's paper, which represents the typical maximum storm as extending vertically to near the stratosphere with about one third of its height indraft, one third updraft, and the top one third outdraft of that part that gets away. All the precipitable moisture does not finally fall out as rain.

Fig. 17 appears to be entirely empirical based largely on visual appearance, mainly as seen in the miniature model of a summer thunderstorm. It seems to be a concept that has been generally accepted for a long time.

The curve, W_E, of effective precipitable water as related to surface dew point is another empirical relation, justified no doubt because it works within the limits of the data from which it was derived.

The limits of the surface dew points are described by Messrs. Showalter and Solot. The upper limit is about 80° F, which is about the maximum possible over the ocean from which the moisture blanket originates. These investigators find that it will not become less than 50° no matter how far the moisture is carried inland.

Just how the maximum probable dew point for the season is derived for a particular region is not indicated, but the authors appear confident they have approached the ultimate. The reader is referred by Messrs. Showalter and Solot to the Sacramento report for discussion of that feature.

The second step in the procedure is to estimate the velocity of inflow. Local anemometer records yield information as to wind velocities near the ground. Velocities in the upper part of the lower one third of the cell representing inflow are derived from the records of temperature and pressure at the 5-km level in the rear of major storms. A complete survey of the extreme minimum temperatures and pressures at that elevation has been made and is being kept up to date, according to Messrs. Showalter and Solot, who state that the maximum rate of upper air inflow is assumed to be reached in most of the major outstanding storms (20*b*)—another ultimate. These men have outlined briefly the procedure involved to estimate the geostrophic wind velocity at that elevation.

The third and final step is to estimate how long the storm will last. Apparently, Messrs. Showalter and Solot failed to find meteorological relations that can be evaluated readily. Resort is made largely to the statistical procedure of the hydrologists in which depth of precipitation, in inches, is plotted against duration in time. Both papers show such curves.

The reasons given for making these studies are twofold. The maximum storm as developed statistically by the hydrologists may be larger than actually can fall from the skies. Messrs. Showalter and Solot do not cite any such instance but it would be interesting if they had. Second, these studies give some measure of control in storm transposition. At least one is less likely to transpose a storm entirely out of its meteorological region. Storm transposition does not seem to be encouraged by these men. Mr. Bernard uses it cautiously. It is being supplanted by area-duration-depth data.

L. K. SHERMAN,[9] M. AM. SOC. C. E.[9a]—One of the tests, as to whether the occurrence of the ultimate maximum storms presented by the author are within the realm of reasonable probability, is furnished by computing the runoff that such storms would produce. Such computed runoff per square mile of basin may be compared by analogy with the large range of recorded runoffs. In any event, it is the amount of the possible flood that is wanted. The storm is simply a means to estimate the flood magnitude.

When he was called upon to estimate the maximum possible flood from the Red River basin above Denison, Tex., the writer utilized the author's maximum design storm shown as curve K in Fig. 8. This depth-duration curve, drawn to a natural scale, with more detail, is shown in Fig. 23, taken from a report of

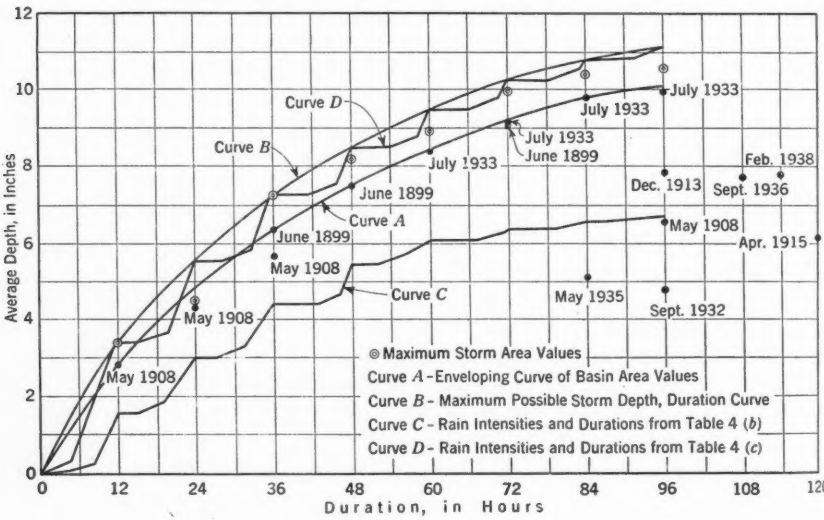

FIG. 23.—REVISED DEPTH-DURATION CURVES, RED RIVER BASIN ABOVE DENISON, TEX.

the U. S. Weather Bureau (33). Curves C and D were plotted in Fig. 23 by the writer.

The derivation of runoff from this rain depth-duration curve B presents some problems not encountered in deriving runoff from an observed storm. The storm patterns, intensities, and durations of rainfalls are unknown in this case.

As a guide in deriving these unknown quantities, the writer utilized the data from the largest storm of record which fell on the Red River basin above Denison—the storm of May 21–25, 1908. The data derived from the records of this storm are presented in Table 4(*a*). The 4-day storm period was divided into eight periods of 12 hr. Each 12-hr period was computed as if it were a separate storm. In this way, cognizance was taken of the continuous changes

[9] Cons. Engr., Chicago, Ill.

[9a] Received by the Secretary May 27, 1943.

TABLE 4.—Record of Storm on the Red River Basin, above Denison, Tex., May, 1908; Drainage Area, 38,400 Sq Miles

Item No.	Description	Order of Magnitude: 1	2	3	4	5	6	7	8	Total
	(a) Observed Data									
1	Order of events	2	3	4	1	5	6	7	8	....
	Rainfall, Wet Area:									
2	In inches	1.51	1.48	1.39	1.06	0.62	0.27	0.18	0.11	6.62
3	Percentage of basin	100.0	97.4	83.1	40.4	67.0	45.0	28.1	9.2	....
4	Rainfall, weighted, on the basin, in inches	1.51	1.44	1.15	0.43	0.42	0.12	0.05	0.01	5.13
5	Infiltration capacity, f, in inches per hour	0.30	0.23	0.23	0.30	0.23	0.20	0.20	0.20	....
	Duration, in Hours:									
6	Noneffective	3.9	4.0	3.9	3.7	4.1	3.9	5.0	7.0	35.5
7	Effective	3.9	5.1	4.9	1.9	3.8	1.3	0.0	2.0	22.9
8	Depth of noneffective rainfall, in inches	0.26	0.26	0.31	0.20	0.24	0.18	0.15	0.15	1.75
9	Average effective-rain intensity, i, in inches per hour	0.37	0.25	0.26	0.45	....	....	....	....	....
10	Basin runoff, in inches	0.528	0.312	0.250	0.094	0.121	0.034	0.001	0.000	1.33
	(b) Rainfall in Item 2 Is Assumed to Cover the Entire Basin; and f in Item 5 Is Taken as $f_c = 0.20$									
	Rainfall, Wet Area:									
11	In inches	1.51	1.48	1.39	1.06	0.62	0.27	0.18	0.11	6.62
12	Percentage of basin	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
13	Rainfall, weighted, on the basin, in inches	1.51	1.48	1.39	1.06	0.62	0.27	0.18	0.11	6.62
14	Infiltration capacity, f_c, in inches per hour	0.20	0.20	0.20	0.20	0.20	0.20	0.20	0.20	0.20
	Duration, in Hours:									
15	Noneffective	3.9	4.0	3.9	3.7	4.1	3.9	5.0	7.0	35.5
16	Effective	3.9	5.1	4.9	1.9	3.8	1.3	0.0	2.0	22.9
17	Depth of noneffective rainfall, in inches	0.26	0.26	0.31	0.20	0.24	0.18	0.15	0.15	1.75
18	Average effective-rain intensity, i, in inches per hour	0.49	0.29	0.30	0.49	0.36	....	....	....	....
19	Basin runoff, in inches	0.76	0.41	0.42	0.47	0.17	0.074	0.0053	0.0007	2.31
	(c) Maximum Storm on the Red River Basin (Writer's Derivation of Intensity and Durations Based on Curve B, Fig. 23)									
	Rainfall, Wet Area:									
20	In inches	....	....	....	....	....	....	....	....	11.1
21	Percentage of basin	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
22	Rainfall, weighted, on the basin, in inches	3.4	2.2	1.7	1.2	1.0	0.7	0.6	0.3	11.1
23	Infiltration capacity, f_c, in inches per hour	0.20	0.20	0.20	0.20	0.20	0.20	0.20	0.20	0.20
	Duration, in Hours:									
24	Noneffective	4.45	4.45	4.45	4.45	4.45	4.45	4.45	4.45	35.5
25	Effective	7.1	4.6	3.5	2.5	2.1	1.4	1.2	0.6	23.0
26	Depth of noneffective rainfall, in inches	0.27	0.27	0.27	0.27	0.27	0.27	0.27	0.27	2.16
27	Average effective-rain intensity, i, in inches per hour	0.45	0.42	0.41	0.37	0.35	0.31	0.28	....	....
28	Basin runoff, in inches	1.72	1.02	0.73	0.44	0.32	0.13	0.08	0.01	4.45

in the position of storm intensities, areas wetted, and durations of effective, noneffective, and no rain.

In this 1908 storm, the point of maximum rain intensity started at Childress, Tex.; then proceeded to Hobart and Pauls Valley, Okla; thence south and west along the Red River back to Childress; and finally moved to Harrington, Okla., and Denison. The movement was always in a clockwise direction. The four highest spot rainfalls in the 12-hr periods were 4.29 in., 4.42 in., 4.27 in., and 4.39 in. There were forty-four rain recording stations in and about the basin, with four first-class stations outside. Mass curves of rainfall were made for most of the rain stations by the U. S. Weather Bureau. With a template set to the angle of f (Item 5, Table 4(*a*)), the writer derived the time durations of rains given in Items 6 and 7, Table 4(*a*). The weighted averages used in deriving runoff were obtained with Theissen's method. For moving storms the writer prefers this method to isohyetal maps. The averaged 12-hr amounts and durations in Table 4(*a*) actually represent a large number of downpours and lulls, varying from zero to very high intensities throughout the 12 hr. They are grouped into two classes: Effective and noneffective rain.

Table 4(*b*) assumes that the storm of May, 1908, covered the entire basin of 38,400 sq miles to the average depths covered on the wetted fractional areas found in Table 4(*a*). It further assumes that the ground has been wetted by antecedent rainfall so that its infiltration capacity has been reduced to f_c, the ultimate minimum capacity of the basin for the early growing season, in which the major floods have occurred. Durations of rain are kept as per the actual storm on the basin. The resulting rain amounts have been plotted as curve C in Fig. 23. The assumptions here made are conservative, as evidenced by the fact that the amounts of rainfall are under the 12-hr amounts given by the U. S. Weather Bureau, marked "May, 1908." These latter points represent amounts from the May, 1908, storm when transposed to its most effective position on the basin.

Table 4(*c*) is a computation based on the maximum rainfall depth-duration curve B of the U. S. Weather Bureau. This rainfall totals 11.1 in. average depth over the basin. This is more than twice the amount of the largest storm of record.

Tables 4(*a*) and 4(*b*) indicate that depths of noneffective rain, in the largest observed storm on the basin, followed fairly uniform averaged amounts in each of the 12-hr periods. The observed average intensity of all the noneffective rains at each of the stations on the basin was about 0.05 in. per hr or 0.06 in. per hr. The total period of noneffective rain in the maximum storm probably will be longer than that of 1908. How much longer is unknown. It is not reasonable to suppose that the maximum storm will have a total duration less than that of 1908. Any assumption of increase in the duration of noneffective rain will reduce the volume of runoff. Therefore, the writer has assumed that the average intensity of noneffective rain will be 0.06 in. per hr and that the duration will be 35.5 hr, the same as in 1908. This gives 35.5/8 = 4.44 hr for each period. At 0.06 in. per hr the depth of noneffective rain is 0.27 in. This appears in Item 26, Table 4(*c*).

Following the same line of reasoning, the writer has adhered to the total duration of effective rains (23 hr) as it existed in the largest storm of record. However, this value is subject to comparison with the resulting average intensities. The average effective-rain duration for a 12-hr period is $22.9/8 = 2.86$ hr. Item 7, Table 4(*a*), and Item 16, Table 4(*b*), indicate the the effective rain durations are longer than the average for the larger rains and shorter than average for the lesser rains. Accordingly, the writer has prorated the average duration in the proportions of each given 12 hr of effective rain depth to the average depth. The average effective rain depth is $11.1/8 = 1.39$ in. Prorating to derive Item 25, Table 4(*c*):

$$2.86 \text{ hr} \times 3.4/1.39 = 7.1 \text{ hr}$$
$$2.86 \text{ hr} \times 2.2/1.39 = 4.7 \text{ hr}$$
$$2.86 \text{ hr} \times 1.7/1.39 = 3.5 \text{ hr, etc.}$$

The derivations of infiltration capacities f for the basin were made from a number of observed rainfall-runoff records including the 1908 storm.

Values of average effective-rain intensity for 12 hr over the basin were derived thus (using Table 4(*a*) as an example):

$$\frac{\text{Item 4} - \text{Item 8}}{\text{Item 5}} = i \text{ or Item 9}$$

Runoff from the basin in Tables 4(*a*) and 4(*b*) was derived by taking the sum of the rain excess on each Theissen subarea multiplied by its percentage of the entire area. Runoff in Table 4(*c*) was found thus:

$$\text{Item 22} - \text{Item 26} - \text{Item 25} \times \text{Item 23} = \text{Runoff or Item 28}$$

Runoffs, in cubic feet per second, were derived by the unit-hydrograph method; 12-hr distribution graphs were used against the eight derived depths of runoff in the sequence of their occurrence. In the cases where f_c was a constant, the sequence or order is immaterial.

The Red River basin above Denison includes two major streams which unite just above Denison. One is the Red River proper with a drainage area of 30,600 sq miles, and the other is the Washita with 7,700 sq miles. Hydrographs from several storms showed that peak from the Washita sometimes followed and sometimes preceded the Red River peak, thereby showing that synchronization of the two-peak flow was possible. Accordingly, separate unit hydrographs were derived for the Red and Washita rivers and the two combined to derive the ultimate possible peak flow.

The peak of the flood hydrograph, from the design storm (curve B, Fig. 23) at Denison, will be 1,500,000 cu ft per sec or 39 cu ft per sec per sq mile. This is based on the possible synchronization of the Red and Washita peaks.

The more probable peak flow, without synchronization, will still range from 1,100,000 to 1,300,000 cu ft per sec. The peak flow over a spillway, of course, will be reduced in accordance with available flood storage back of the dam.

The peak runoff of 1,500,000 cu ft per sec does not mean that the total volume of the maximum flood is increased. That volume remains the same whether the peaks synchronize or not.

In combining unit hydrographs from different areas, the distribution graph must be expressed in inches and not in percentages.

The writer is of the opinion that: (*a*) The author's "design storm" is what it purports to be—"the maximum possible storm"; (*b*) it should be used in the design of all structures which, in the event of failure, would involve large loss of life and property damage; (*c*) the values for runoff thus derived call for no additional factor of safety, since that factor has been cared for by the concatination of possible events; and (*d*) the author's "design storm" furnishes far more reliable limits than any data derived by statistical analysis. The latter, based on a long-time record, does furnish a possible check on the engineer's computations and may aid him in deciding whether a particular structure warrants the elimination of certain remote hazards.

Bibliography.—

(20) **"Computation of the Maximum Possible Storm,"** by A. K. Showalter and S. B. Solot, *Transactions*, Am. Geophysical Union, 1942, Pt. II, pp. 258–274. (*a*) Fig. 2, p. 261. (*b*) p. 259.

(32) **"Computation of Depth of Precipitable Water in a Column of Air,"** by S. B. Solot, *Monthly Weather Review*, April, 1939, pp. 100–103.

(33) **"Maximum Possible Precipitation Over the Red River Basin Above Denison, Tex.,"** by Merrill Bernard and others, U. S. Weather Bureau, October 26, 1939.

Correction for *Transactions:* In January, 1943, *Proceedings*, page 142, change reference (20) to read: **"Computation of the Maximum Possible Storm,"** by A. K. Showalter and S. B. Solot, *Transactions*, Am. Geophysical Union, 1942, Pt. II, pp. 258–274.